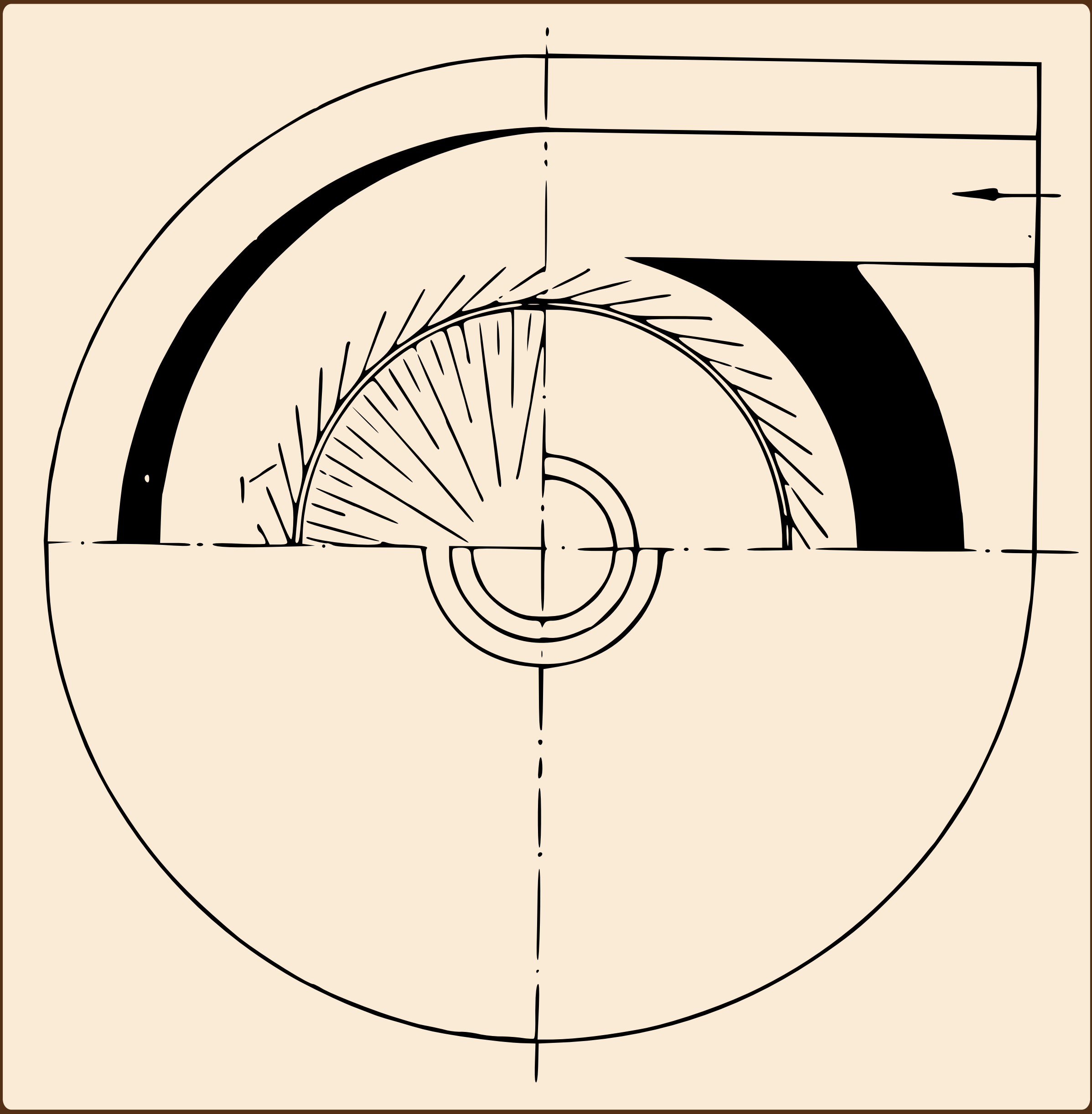
Collected Papers of P.L. Kapitza

Edited by D. Ter Haar

VOLUME III



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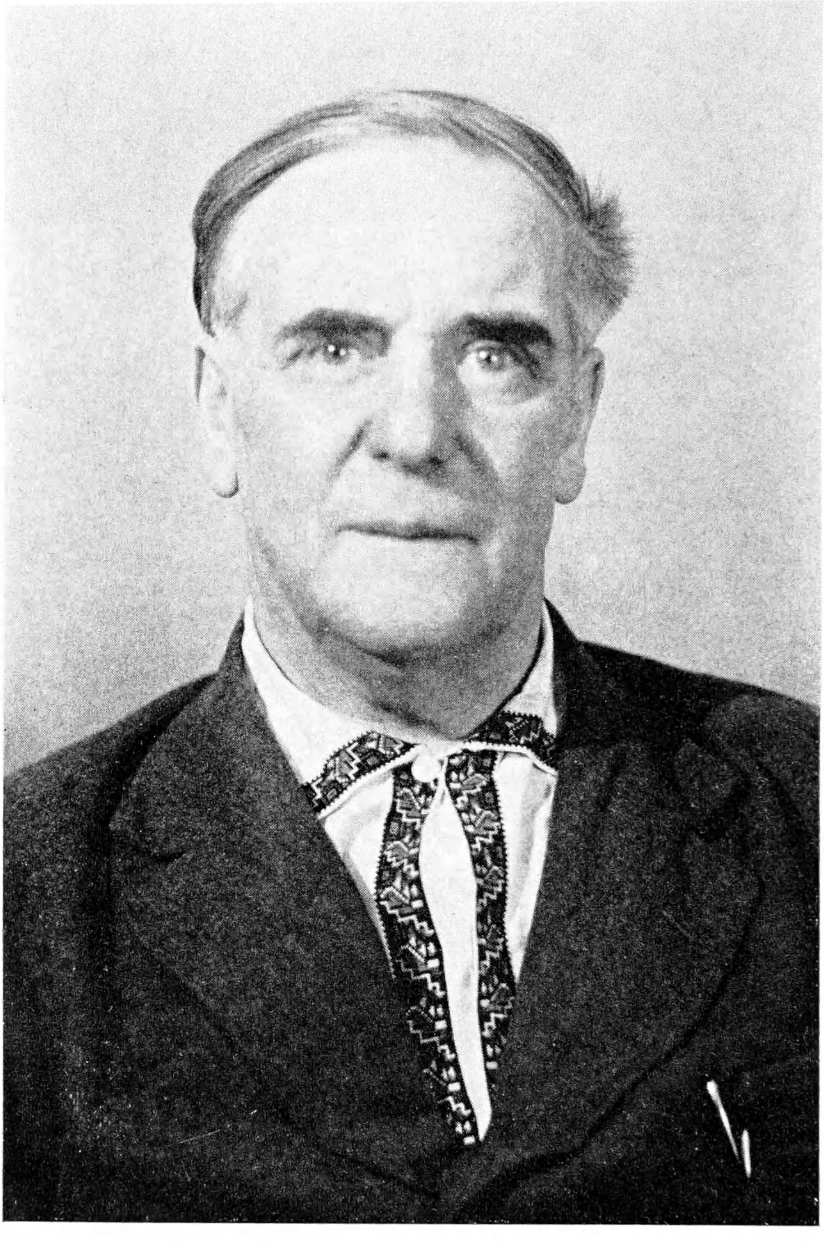

P. L. Kapitza

COLLECTED PAPERS OF
P. L. KAPITZA

(in three volumes)

VOLUME III

Edited by

D. TER HAAR

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PREFACE

THE present volume completes the publication of the *Collected Papers of P. L. Kapitza.* It contains a few semi-popular scientific survey papers as well as papers on topics of a more general nature.

I should like to express my gratitude to everybody who has assisted me in getting this collection together, but my special thanks are due to Mr. P. E. Rubinin for his ceaseless help in locating various papers and in providing copies of the text and of the various illustrations. Thanks are also due to Professor Aa. Bohr for providing me with a photograph of his father, to Dr. and Mrs. Nikolič and Mr. Blunn for translating from the Russian and to Macmillan and Company Ltd., the Oxford Junior Scientific Club, the Royal Society and the American Association for the Advancement of Science for permission to reprint papers which first appeared in *Nature*, the *Transactions of the Oxford Junior Scientific Club*, the *Proceedings of the Royal Society*, and *Science*.

Oxford, September 1966 D. TER HAAR

INTRODUCTION

In this introduction we shall very briefly indicate the contents of the various papers contained in the present volume. First of all, there are a few papers describing in semi-popular terms different lines of research in which the author was interested at the time of writing. As often as not they are the text of talks given to an audience which did not consist purely of specialist-physicists. The first one[3]* of these is the text of a talk given to an undergraduate audience at Oxford. It deals with Kapitza's work on strong magnetic fields which took up such a large part of his research during his time at Cambridge. Then there are two papers[12, 18] dealing with research on liquid helium for which Kapitza received a Stalin Prize. It is interesting to compare these two papers as the first one was written when the research was still going on while the second one was written after it had been brought to an end by the war and the removal of the Institute of Physical Problems from Moscow to Kazan.

A second lot of papers are bibliographical, but are at the same time used by Kapitza for historical, philosophical, and sociological discussions. Firstly, there are four papers [5, 6, 11, 31] on Rutherford of which the first is one of a number of contributions to the In Memoriam issue of *Nature*. The third article is the entry in the *Great Soviet Encyclopaedia,* while the second paper is the text of a memorial lecture given by Kapitza in Moscow. Secondly, there is a brief paper[19] on Newton given as a representative of the Royal Society to a meeting of the Academy of Sciences celebrating the tercentenary of Newton's birth. In 1956 on the occasion of the fact that Benjamin Franklin was born 250 years ago, Kapitza[20] discussed his scientific work. In 1961 he did the same[24] for Lomonosov, the Russian poet and scientist who for so long was unjustly completely forgotten and after whom Moscow University is named. On the occasion of the 85th birthday of Langevin, Kapitza described[28] his activities both as a scientist and as a statesman. Finally, we mention here a brief obituary note[27] on Niels Bohr.

Apart from a very brief account[1] of the cod-liver-oil industry—the first paper written by Kapitza and based upon a trip to the far North—a book review[2] discussing the importance of magnetic investigations, a short account[9] of an experiment on stereoscopic films, and a discussion[21] with Bertrand Russell on the banning of atomic weapons by mutual agreement, the remainder of the papers in this volume all deal with some aspect of the best way to organize science. Most of those papers are again based upon talks given to different audiences. For instance, in two papers[4, 16], given before the Academy of Sciences of the U.S.S.R., Kapitza describes the organization of his own institute, the Institute of Physical Problems of the Academy of Sciences of the U.S.S.R. In the second of these reports he mentions the liquefaction of air, a theme to which he returns many times[7,8,17]. Kapitza was concerned here with producing cheaply oxygen for industrial purposes and had shown both theoretically and experimentally that the way to do this was by means of a turbine expansion machine. From his papers it is clear that there were many obstacles to overcome before this process was industrially viable.

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* The numbers refer to the chronological list on p. v.

In the remaining papers[10,13,14,15,22,23,25 26,29,30] Kapitza discusses from various points of view how, in his opinion, Science can best be organized so as to benefit society most. Although he emphasizes the difference between socialist and capitalist countries, one does not get the impression that in actual fact his solutions could not just as well be applied in the West as in the U.S.S.R. Particularly interesting in those papers are his discussion[25] of the difference between theorists and experimentalists and a discussion[26] of future developments both organizational and research-wise in Science.

1. COD-LIVER OIL

It is known that cod-liver oil is considered one of the best remedies for building up an organism's strength after being run down by sickness. The oil is produced in Russia in the far North, beyond the polar circle, under the midnight sun, on the harsh Murmansk coast. The produce from which the oil is extracted is the *liver* of the cod, which is caught in the Arctic Ocean by the Pomors.

The Pomors are good Russian seafarers who from early childhood are used to the sea; they know its caprices and secrets. In winter they inhabit the shores of the White Sea, but in summer they leave and settle near Murmansk where they are occupied solely in catching cod.

They dwell in *stanovishchakh* along the shores of the bays. On the site, amongst the filth and refuse, factories are set up, i.e. small factories where the oil is clarified.

How Cod Is Caught

The cod is a fish of fairly large dimensions, ordinarily no more than one and a half arshin (42 in) in length, and up to one and a half pood (54 lb) in weight. It is caught on the so-called *yarus*.

This tackle consists of a line, about a quarter of an inch thick, to which other thinner lines are tied, spaced out 1 sagene (7 ft) apart; hooks are attached to their end. Each hook is baited with a small fish.

The whole tackle is usually from seven to fifteen verst' ($4^1/_2$–10 miles) in length and, consequently, there are three to six thousand hooks. One can easily imagine how long and difficult it is to string up and bait the *yarus*. The work is done by *zuiki*—so are called the boys of 8–12 years who are paid three copecks for baiting 150 hooks.

After the baiting the *yarus* is loaded onto small boats which are not at all safe to sail. In any event it requires great self-confidence to fight storms on such a boat and, above all, an excellent knowledge of the ocean and a sure hand.

But though the Pomors have all these qualities, every year the ocean claims many brave fishermen.

They go after the cod far out to sea, sometimes forty verst' (25 miles), and there they pay out the tackle which takes from two to three hours.

Because of its weight the *yarus* lies on the ocean bed 150 to 600 fathoms deep.

When it has been on the ocean bed for about six hours, the Pomors start pulling it in. This is difficult and tiring work .... The hooks catch not only cod,

Капица, Рыбий жиръ, *Аргусъ* **10**, 76 (1913).

but also other kinds of fish: haddock, sometimes even 6-foot sharks and giant crab. The fish are killed on the spot—they are stunned and heaped in the bottom of the boat.

Ordinarily they catch 30 to 100 pood (1–3 cwt.) of cod at a time. On return to land they immediately begin cleaning the fish. The photograph (Plate 3) shows two Pomors in this occupation. The first man cuts the heads off, casts them away, and passes the carcasses to the other man; he rips the cod open and takes out its liver and intestines. The liver is laid in a basket on board, but the intestines are thrown away.

PRODUCTION OF COD-LIVER OIL BY THE POMORS

The cod's liver (*maksa*) is the part of the cod from which the oil is extracted. It is light brown in colour, consistent in texture, and it weighs from 3 to 5 lb, that is, ordinarily one fifteenth of the weight of the cod itself.

Before the factory system of producing the oil, the Pomors used to do the pressing themselves. This was done very primitively. They packed the *maksa* in small barrels where it was left to stand. After a while it started to putrefy.

Owing to the rotting, the fat cells and oil dripped to the bottom of the barrel where it was tapped off through a special hole. There is no need to say that this was an unsatisfactory method since the oil still contained the products of putrefaction of the liver and these began to putrefy too, wherefore, despite its transparency, the smell of the oil was strong. Furthermore, the method was inefficient since not all the oil was extracted and much was completely wasted.

This system has now been almost entirely replaced by a newer method—the factory method.

FACTORY PRODUCTION OF COD-LIVER OIL

Receiving the maksa, the producer firstly cuts the gall bladder from the liver and washes it in water. The maksa is then placed in special boilers for boiling out the oil.

These boilers stand in sheds, usually on the seashore. They a have double bottom and double walls. So it is as if a small boiler is inside a larger one. The space between the boilers is filled with water. To tap off the oil, the inner boiler has a copper stopcock. The boilers are made from sheet iron; the firebox is beneath them. Of the cleanliness of these boilers it is no use speaking; they are never cleaned and they are not washed. They commence to heat the maksa in them.

Firstly, they raise the temperature to about 50°C.

At this temperature with continual stirring the oil begins to separate off and flow downwards. It is tapped off into a bucket.

The first grade is the most expensive and the best; it is light yellow in colour and the smell is less than with other grades. It is called white cod-liver oil; only one quarter as much is obtained compared with the other grades and

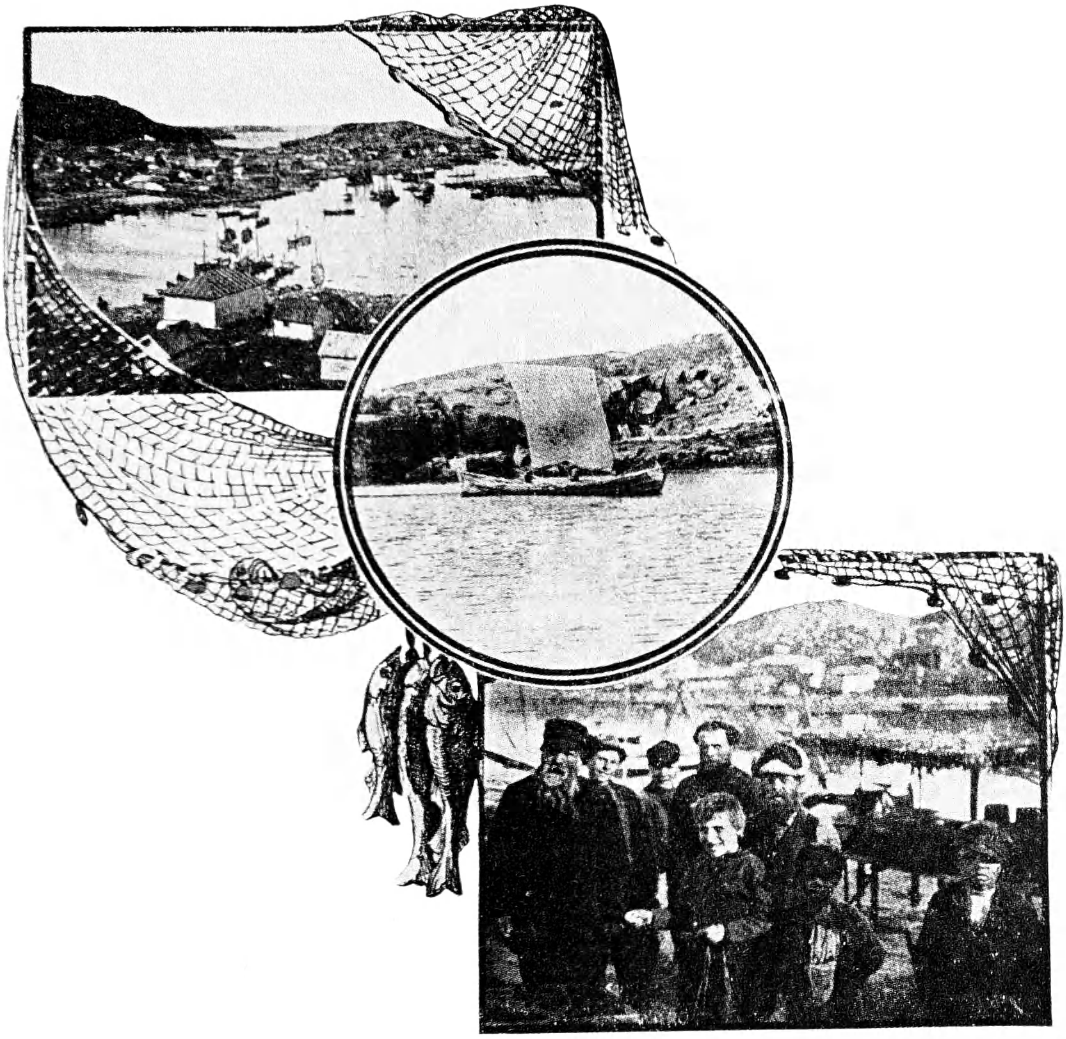

PLATE 1. Pomor fishing boat sailing out to sea for cod. View of a "stanovishch". Factories on the left- and right-hand shores (factory where the oil is purified). Group of Pomors unravelling the yarus. (on the right the barrels of oil stand on decks ready for shipment.)

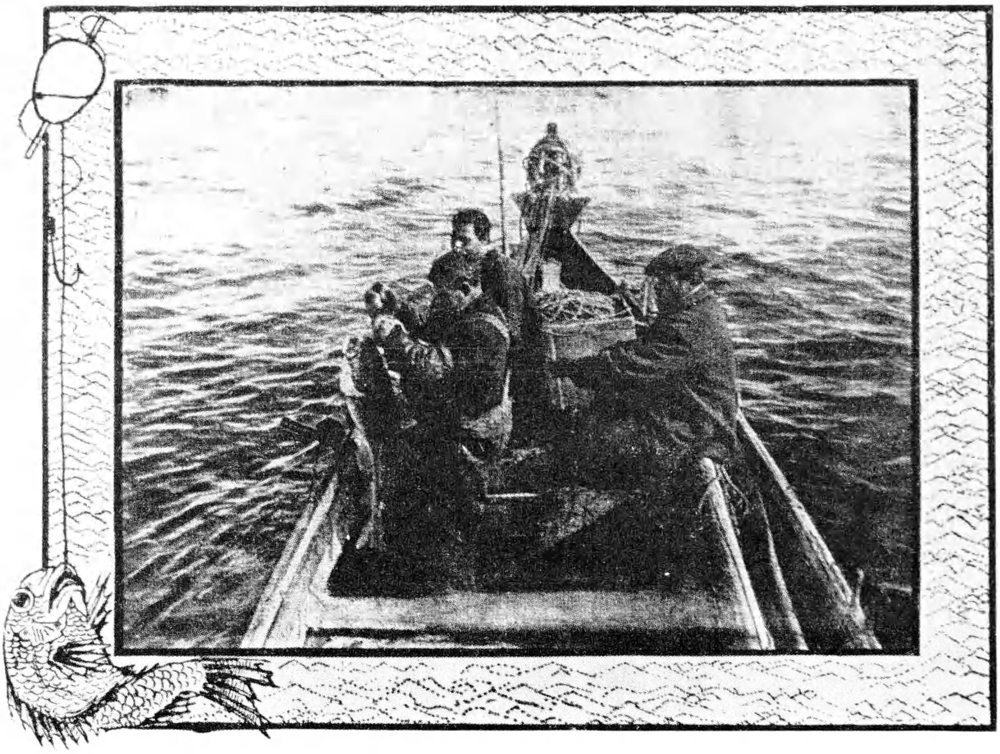

PLATE 2. Pulling out the cod.

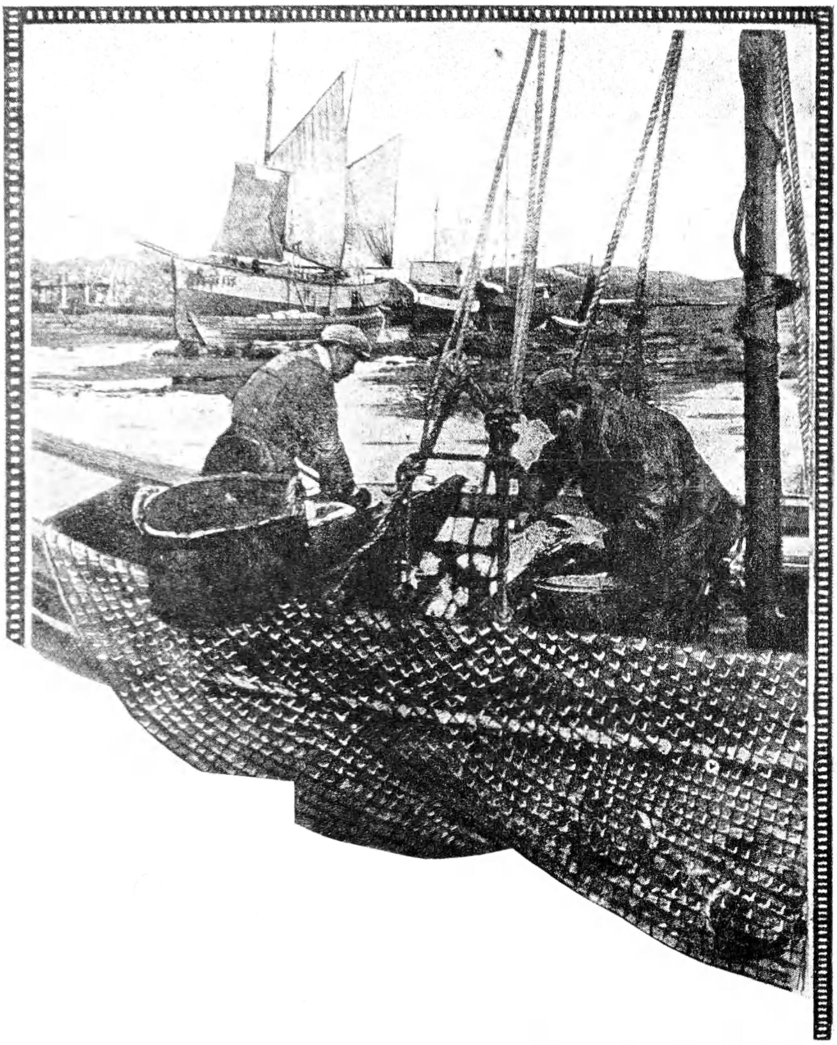

PLATE 3. Cleaning the cod. In the distance ships taking the cod liver to Arkhangelsk.

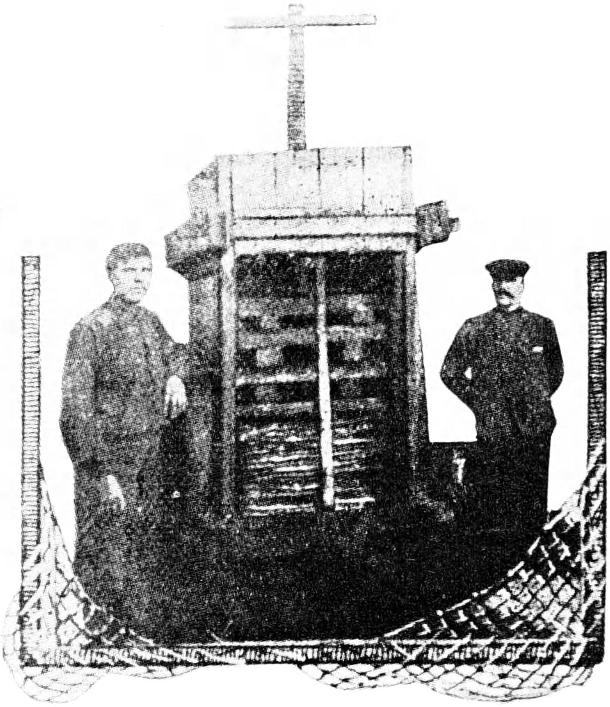

PLATE 4. Press for extracting the oil.

it goes exclusively for medicinal purposes. When all the white oil has come off, the boilers are heated until the water between the walls begins to boil and thus the maksa is at 100°C. It is difficult to describe the stench which arises from the boilers. When I first went into the building to take photographs, my companions and I felt sick. Each boiler is tended by a worker to stir the maksa so that it is heated uniformly.

When the maksa has boiled sufficiently, and this takes several hours, the oil is again tapped off. But it is markedly different from the first grade. It is darker, not so transparent, and it smells stronger than the white oil. It is called yellow oil, and it is hardly ever used as medicine; it is used mainly for technical purposes, for instance, for greasing hides in the leather trade, and so on.

The dark colour and strong smell are due to bile acid impurities. The remains of the maksa are not wasted; they are stuffed in cloth bags and pressed under a special press.

This is done to remove the last remains of oil from the maksa. One such press is shown in the photographs (Plate 4).

A worker stands on top and turns the screw by the handle, thereby pressing the bags and squeezing out the oil into the bucket underneath the press. This third grade of oil is very dark, almost brown, and it is called red fish oil. It is used solely for technical purposes.

The remains of the maksa, in Russia, are thrown away, but in Norway and North America where cod-liver oil is also produced, they dry it in special furnaces along with other wastes of the fish industry, crush it and produce fish meal which serves for fertilizing the soil.

The hot oil is left to stand, then filtered through canvas, and finally poured off into small barrels. The barrels are then bunged tight and loaded together with the cod on large steamships which ship them to Arkhangelsk, and thence they are distributed throughout Russia.

2. THE FUTURE OF MAGNETISM

It is probably recognized by every one that the main interest in physics of to-day lies in the study of the atom. Much of the information with regard to the atom has been obtained by studying spectra; chemistry, magnetism, X-ray scattering, etc., play only a subsidiary part. We must admit, however, that our spectroscopic material is now more or less exhausted, and that we must look for fresh sources of information.

Much may be said in support of the opinion that magnetism will open a new way by which to approach the study of the structure of the atom. The atom is essentially an electromagnetic system which consists of a positively charged nucleus with negative electrons revolving round it. If the atomic number of the atom is given with its nuclear charge, and if the electrons arrange themselves round the nucleus in a definite way, then this arrangement of the electrons practically fixes all physical and chemical properties of the given element.

The magnetic field is probably the only practical weapon by means of which we may hope to change the motion and arrangement of the electrons in the atom, and thus influence all the physical and chemical properties of the atom. In only a very few cases at the present time do we find that the influence of the magnetic field on the properties of the atom is noticed. This is because the influence of the available fields is too weak to produce a marked change in the properties of the atom, and our present methods of magnetic research are not sufficiently refined to study them. The most easily observed magnetic phenomenon is the Zeeman effect, and this has a tremendous influence on the present theory of the structure of the atom.

It is possible that the difficulty of experimenting in magnetism, and the small amount of trustworthy experimental work done, account for the fact that magnetism has been somewhat neglected. During the last few years, however, a considerable amount of research has taken place, and new methods of approaching the subject have been developed, and we now have to recognize a marked advance in magnetism.

It is on this account that we have to welcome Dr. Stoner's book, especially as it attempts to give an account of our present knowledge of theoretical and experimental magnetism from the point of view of its relation to the structure of the atom.

The task of writing such a book, the counterpart of which has not before been published in any language, is indeed difficult; and in his preface Dr. Stoner himself admits the difficulty. The material which an author of a book on

P. Kapitza, The future of magnetism, *Nature*, **119**, 809 (1927); A review of ***Magnetism** and atomic structure* by E. C. Stoner (Methuen, London 1926).

magnetism has at his disposal is very large, but the great amount of contradiction in experimental as well as in theoretical work makes the problem of selection very considerable.

In his preface Dr. Stoner states, " Prominence is given to the work which is thought to be of most important and lasting value," and the difficulty of selection may be illustrated by the following example. In Chapter XII (p. 273, para. 4), Glaser's experiments are described at length, and in Chapter XV they are well discussed. A few weeks before Dr. Stoner's book appeared, however, Glaser's experiments were repeated by Lehrer by a more refined method (*Z. für P.*, vol. 37), and it was shown that the increase of atomic diamagnetic susceptibility at low pressures in diamagnetic gases, as observed by Glaser, is almost certainly due to experimental error. This example is given, not to criticize Dr. Stoner's work, but simply to illustrate the difficulty of his undertaking.

On the whole, we must agree with Dr. Stoner's choice and with the manner in which he has put together the material at his disposal. The experimental and theoretical parts of the book are well divided; it is free from heavy mathematics; the subject is well brought up-to-date, and the references which follow each chapter are very valuable.

Dr. Stoner also gives a brief account of electrodynamics, the quantum theory, and other work which has been done on the structure of the atom, so as to enable the unprepared reader to follow the main subject of the book. We scarcely consider that such a brief account is sufficient to impart the preliminary knowledge necessary to follow the subject of the book—it can be regarded only as a means of recalling certain facts with regard to electrodynamics and the quantum theory to the mind of the reader who is already acquainted with them. It is doubtful whether it is really advisable to include this account in the book.

In general, the book is of more use to the experimentalist than to the theoretical research student. A close study of it reveals one or two slips and misunderstandings, but none of a very serious nature. Attention may be directed to one of these, and that is on p. 196, where Dr. Stoner makes some calculations on the gyromagnetic effect. In this connexion the equations (9·2) are wrong, as the author puts the sign of equality between two expressions which cannot be equal. On the following page he himself suggests that this may be "wholly wrong", and so it is.

It is probable that as magnetism attracts more and more attention, and as its importance in the study of the atom increases, we shall soon have more books published on this subject. Dr. Stoner's book is, however, the first on this subject, and we welcome it as a very good commencement and as an important contribution to our present literature on magnetism.

3. THE PRODUCTION OF AND EXPERIMENTS IN STRONG MAGNETIC FIELDS

In the light of modern knowledge we take it that the structural character of the atom is of an essentially dynamic nature; that is, the atom consists of a system in which moving charged bodies move round a central nucleus, the properties of the atom being wholly dependent on the number and orbits of the electrons. We therefore see that these properties, for instance the magnetic moments, coherence forces, spectra, etc., may be changed if we can find a means of disturbing the motion of the orbital electrons. The most efficient way of doing this is by subjecting the atom to an outside magnetic force. The internal magnetic field produced in the atom by the motion of the electrons in their orbits is of very great magnitude, but should it be possible to obtain an external field of the same magnitude it is obvious that the motion of the electrons would be altered very considerably as the coupling energy between them will be of the same order as the perturbation produced by the field; we should then expect to get some very significant results. However, when we come to consider the order of the fields inside the atom, we find that even for the most loosely bound electrons it would be in the neighbourhood of 1,000,000 gauss. As such a field would be about 30 times larger than is usually available in laboratory work the object of my research has been to develop a method for obtaining fields of this order.

The usual way of generating a strong magnetic field is by means of an electromagnet, but the field is strictly limited owing to the fact that the iron gets saturated. An increase in strength can only be brought about by an enormous increase in the weight of the magnet and of the current used. The largest electromagnet ever built is that of Professor Cotton; the diameter of the iron cores is about 1 metre, between the pole pieces there is room for a man to stand, and an immense current is required. The magnetic field only increases very slowly with increasing size of the electromagnet, and even Professor Cotton's magnet will not produce a field much stronger than 60,000 gauss in a space sufficiently large for experiments.

A more successful method was found to be with a coil. Very large currents are required as the magnitude of the field in a coil is proportional to the exciting current. The obvious way, then, of creating large fields is to increase the current, but here we meet with difficulties; not only do we have to provide a source of very high current, but the magnitude of the current is strictly limited by the

P. Kapitza, The production of and experiments in strong magnetic fields, *Trans. Oxford Univ. Jun. Sc. Club* (5), 4, 129 (1931). This is the text of a talk given to the Oxford Scientific Club on Friday, 27 Feb. 1931.

heating which it produces in the coil. One method for reducing this heat effect would be to carry away the heat as it was generated, or alternatively and more directly, by cooling the coil to very low temperatures. The resistance would thereby be reduced very considerably, and even in certain metals to zero when the metal becomes a supraconductor. The difficulty in this case is that the magnetic field produced by the coil will destroy the supraconducting state and also very rapidly increase the resistance to a value very close to that at room temperature. Neither of these methods seem very feasible, and even if carried out most efficiently would not allow a greater field than 50,000–60,000 gauss to be produced. If we assume we can make an efficient coil with an inside diameter of 1 cm it can be shown that in order to produce a field of 10,000,000 gauss a power of 50,000 kW, is required, and in one second the coil will be heated to 10,000°C, and it is obvious that we could not deal with such a large temperature rise.

The main idea of our method of attacking the problem has been to make the time of duration of the field very short, when the coil has practically no time to overheat. The time actually chosen was 0·01 sec.

This condition, of course, created a new set of difficulties: firstly a very large current was required, and secondly all the measurements had to be made in a very short space of time.

Our first experiments were done using accumulator batteries having a very small capacity and a small internal resistance. In this way we were able to produce fields of 100,000 gauss by charging the accumulators for a few minutes and then discharging them in 0·01 sec, but further increase was impossible as it was found difficult to break currents of several thousand amperes sufficiently suddenly.

In our later experiments when larger powers were required we used a single phase a.c. turbo-generator. It is well known that such a machine will give very large impulses of current when short circuited, and this is carefully avoided in usual practice as it might cause a serious accident. Our machine was purposely designed on the opposite lines so that large impulses were obtainable on short circuit. Considerable revision in design and careful calculation were necessary as the electrodynamical forces might easily result in breakdown of the windings. The machine was of the size normally delivering 2000 kW at continuous rating, but on short circuit on the test bench it gave us 220,000 kW. When short circuited on a coil of equal impedance only half of the power will be available; half of it will be lost in the machine and the other half will go to the coil. In this way the required 50,000 kW are obtainable.

Normally the current in such a coil never remains constant, but with a certain design of the armature it was possible to obtain a current wave with a flat top, which gave a constant magnetic field for several thousandths of a second.

The greatest difficulty encountered was that the coils tended to burst owing to the electrodynamical forces trying to enlarge the diameter. We worked out a method of reinforcing the coils with steel bands, and designed a coil of such a shape that the electrodynamical forces together with the reaction forces of the reinforcement would be reduced to a uniform (hydrostatic) pressure on the copper. The force on the outside reinforcement of our present coil reaches a value of 140 tons.

The next problem was the design of a special switch to break the current synchronously with the current wave. As the current was only of 0·01 sec duration, the time available for making and breaking was only a few ten-thousandths of a second, during which time a copper plate had to separate several millimetres from a brush. The acceleration required to move the copper plate 1 kg in weight this distance is about 1000 times that of the gravity field, and the force required is over a ton. An exceptionally strong and carefully designed cam-shaft mechanism is used for this apparatus.

The controls are so arranged that by means of various timing devices the pressing of a single button carries out the experiments automatically, and oscillograph records are obtained giving the currents in the coil, thus measuring the magnetic fields.

We then had to overcome the difficulty of the shock caused by the sudden stopping of the generator. When the machine is short circuited the angular velocity of the armature, which weighs $2^1/_2$ tons, is reduced by 10 per cent in 0·01 sec, and a strong couple results which tends to turn the whole machine on its foundation. To avoid the results of this shock on our measurements, the coil is placed 20 metres from the generator so that measurements are over before the earthquake reaches the coil.

The short time of experiment led to certain difficulties in methods of observing and measuring, but on the whole the loss in time was compensated by gain in the magnitude of the phenomena observed due to the very strong fields; it also has the great advantage that the effect of the temperature change on different phenomena is practically eliminated, as during 0·01 sec the temperature remains more or less constant.

We have so far studied the effects of strong magnetic fields on various phenomena: for instance, in the study of the Zeeman effect we found that the splitting of the lines was so large that an ordinary prism spectrograph could be used having a large luminosity, and the time of exposure could be reduced, to 0·01 sec without interfering seriously with the accuracy of the results.

The study of the change of resistance of various metals in strong magnetic fields proved to be of great interest; in some cases there was an increase in resistance of about 20 to 30 per cent whereas in ordinary fields there is only an increase of a fraction of a per cent. Moreover, we found that with strong fields a linear law of increase of resistance with increase of field obtains, while with ordinary fields the increase of resistance is proportional to the square of the field.

We have also measured the susceptibility of certain metals in strong fields. For this purpose we designed and constructed a special balance having a frequency of about 2000–3000~. As in our case the magnetic forces were magnified about 100 times, this balance was sufficiently sensitive to measure the susceptibilities of most substances.

Another line of research has been the study of magnetostriction. In ordinary fields this phenomenon is only known in ferromagnetic substances, but in strong fields we found it specially marked in various others such as bismuth, tin and graphite which have a crystal structure of low symmetry. In bismuth crystals in strong magnetic fields the crystal increases in length in the direction of the trigonal axis, but decreases perpendicularly to it.

It will be seen that a wide range of research is opened up in the study of various phenomena in strong magnetic fields lasting for a very short time, but that special technique and apparatus are required.

4. THE CONSTRUCTION AND WORK OF THE INSTITUTE OF PHYSICAL PROBLEMS OF THE U.S.S.R. ACADEMY OF SCIENCES

The Institute of Physical Problems was founded by a Government decree dated 28 December 1934; its rather unusual name was chosen to signify that it would not necessarily be concerned with any particular branch of science, but that it would be a relatively small institute studying certain problems whose exact nature and scope would depend on the particular scientists working there. In other words the institute was destined for pure rather than applied science. I use the somewhat unpopular term "pure science" for lack of a better description of what is in fact a well-established concept ("theoretical science" is sometimes used, but all science is theoretical). The only difference between pure and applied science is that the scientific problems of applied science are set by the requirements of everyday life, while pure science leads itself to practical application, for in one way or another scientific knowledge always finds an application to everyday life, even though it may be difficult to foresee how or when this will happen.

The institute was not founded with the aim of discovering and solving new problems of physics. I was invited to take charge of it and to continue there the work I had been doing for 14 years in England. This work dealt with problems of strong magnetic fields and in connection with these, the attainment of low temperatures; consequently these were also the main problems with which the Institute of Physical Problems was to be at first concerned.

I shall first of all touch on some purely constructional matters, and shall describe the principal ideas on which the construction of the institute was based. Our first problem, a very important one, was that of choosing a site. As most physical measurements are made with very sensitive instruments the institute had to be far removed from the many sources of disturbances, occurring in towns: vibrations from traffic, induced currents from tramway lines and from neighbouring radio transmitters, and so on. A great deal of time was devoted therefore to the choice of a site, and I do not think a better one could be found than that now occupied by the institute; in my opinion, the position of the institute is ideal for town conditions, the building being 80 m away from the tram lines and the traffic vibration being reduced to a minimum.

П. Л. Капица, О строительствем и развертывании работы института физических проблем академии наук СССР, *Известия Академии Наук СССР, сер. физ.*, **3**, 265 (1937).

P. Kapitza, The construction and work of the Institute of Physical Problems of the U.S.S.R. Academy of Sciences, *Izv. Akad. Nauk SSSR, ser. fiz.* **3**, 278 (1937).

After a shorthand report of an address to the Physics Group of the Academy of Sciences, 13 March 1937.

The front of the site (Fig. 1) is occupied by residential quarters, and the scientific buildings are situated in the centre of the site; to the left of the site is the Neskuchni Park, to the right is also a park, both of which will never be built on; in front of the site is the street, on its other side the Academy of Sciences is to lay out its botanical gardens. The institute will thus be surrounded on three sides by gardens: along the fourth side is to be built the Andreyev Canal, so that the isolated position of the institute is likely to be preserved for ever.

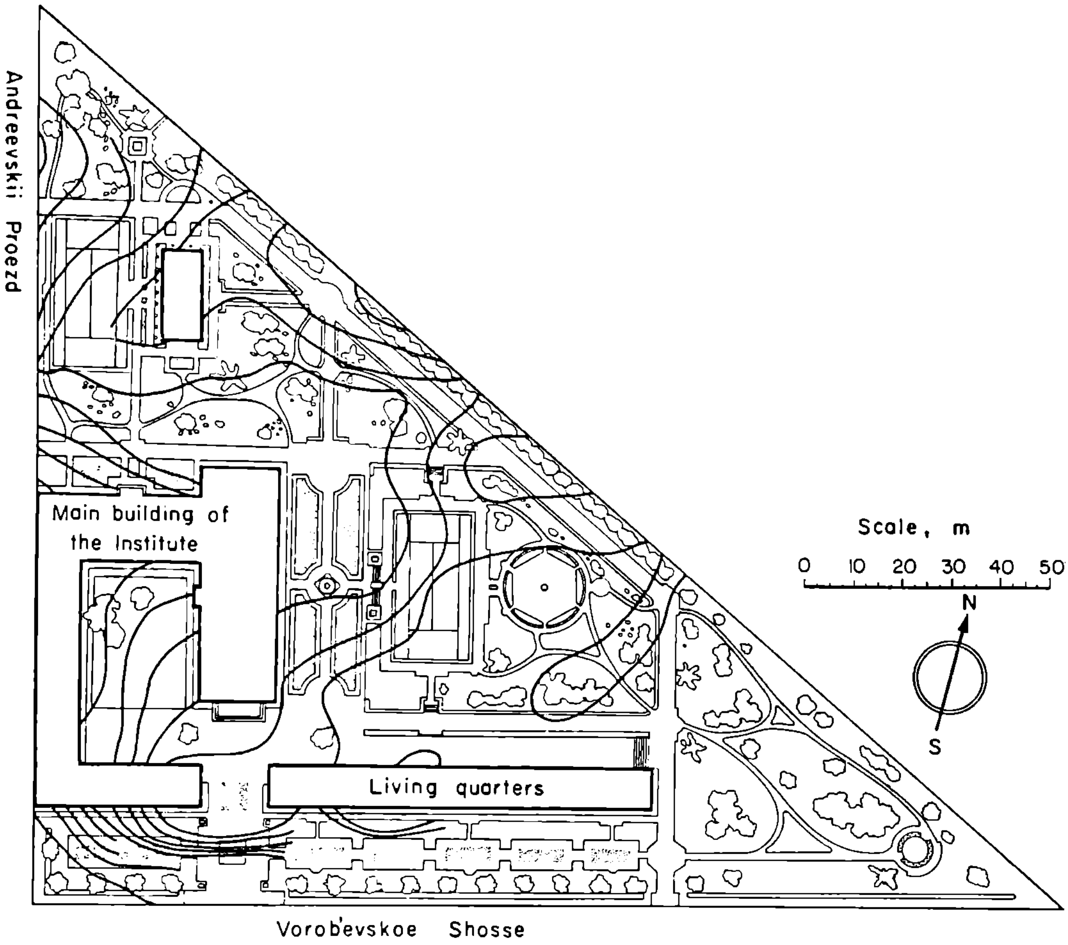

FIG. 1. Plan of the site of the Institute of Physical Problems.

The following plan was adopted in planning the institute. The working laboratory part of the building, in which the experiments are made, is entirely on the ground floor; this is essential in working with liquid helium and hydrogen, which have to be transferred from one place to another, so that it is safer and much more convenient to have everything on one floor (Fig. 2). Moreover the ground floor is much quieter as regards all kinds of vibrations. The ground floor also contains a hall with the liquefaction plants, above which there is a gallery, but there are no rooms over the hall, and the roof is made very light so as to minimize the effect of any explosion which might accidentally occur. The first floor (Fig. 3) of the main building is used only for administration purpose and contains the account departments, various offices and private rooms, a lecture room and the "krasny ugolok" (red corner).

Part of the basement is occupied by the boiler room, while another part is reserved for experiments with particular sensitive apparatus, the floor being built on a special rubber foundation. One room is lined with barium stucco for X-ray experiments, and another is designed for spectroscopic work.

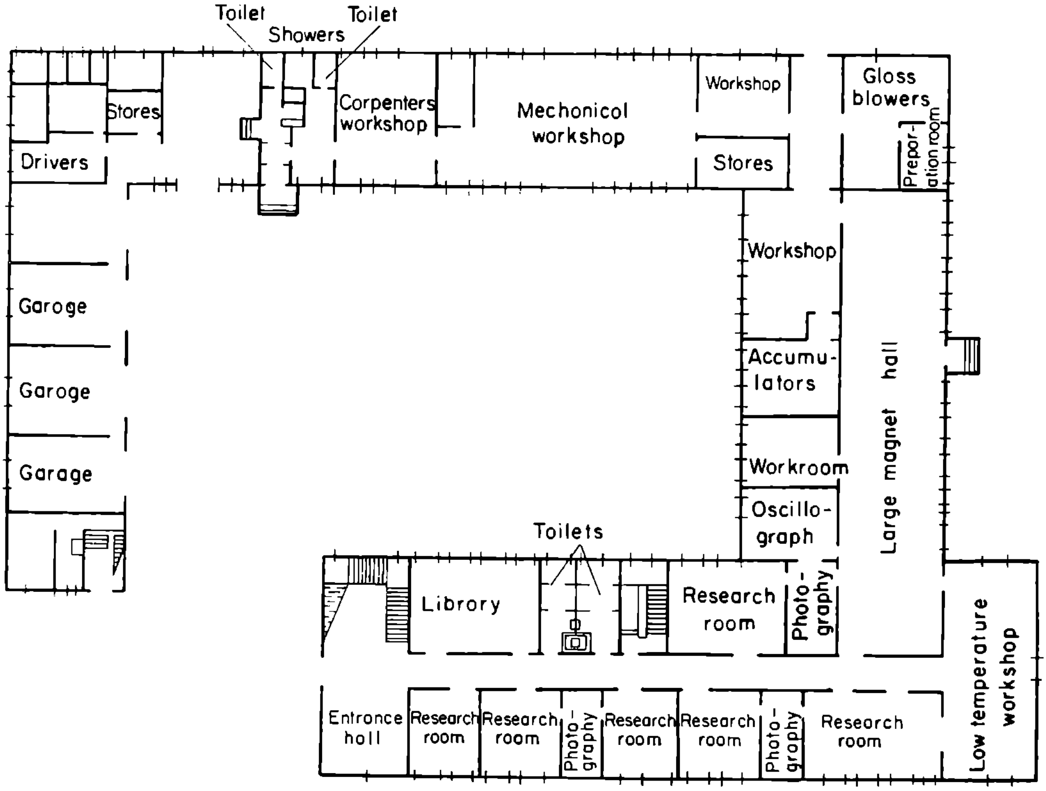

FIG. 2. Plan of the ground floor of the Institute.

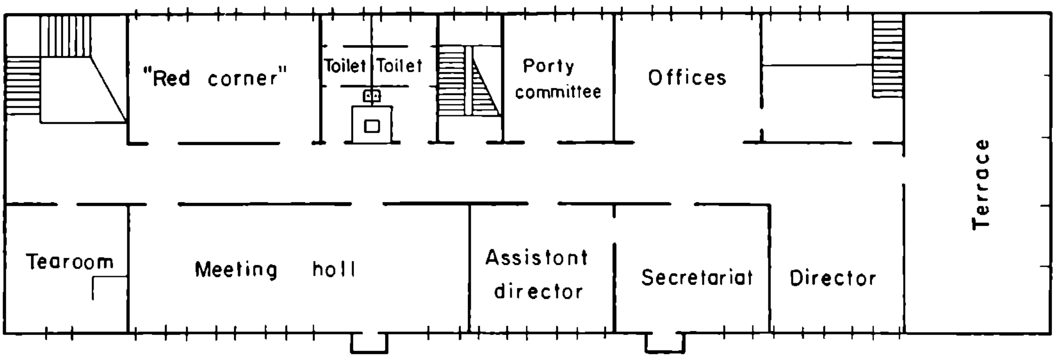

FIG. 3. Plan of the first floor of the Institute.

We have no system of passes in the institute: no visitors can enter directly into the laboratories on the ground floor, since they pass right into the offices

Plate 5. View of the Institute from the main entrance.

upstairs, from which the laboratories can be reached only through the director's private office. On the other hand, the scientific workers of the institute have access from their rooms to the director's office by means of an internal staircase, thus establishing direct communication between the director and his collaborators.

The institute was planned as a self-contained unit, with its own workshops, library, etc. This however is unsatisfactory and is only temporary, for our institute is a part of the whole aggregate of the Academy of Sciences Institutes and when, eventually, these will be built all close to each other, it will be possible to reduce considerably our administrative and business departments. Since our institute will be near all the institutes to be built by the Academy of Sciences there will be no purpose served by maintaining our own complete library as we shall be able to use that of the Academy of Sciences. The same is true of our workshops, which are rather large at present, but for the time being we shall have to maintain our own separate services.

As regards the actual building work of the institute, this took two years, though it should have been finished in one. Although the supply of materials was quite regular and there was never any shortage, the builders were not up to the mark; their work was inadequately organized, and they were unable to finish it in the stipulated period. Moreover the quality of the finished work could be described only as "satisfactory" by the Government Commission—this was a great pity since the design of the building was a very attractive one and should have been carried out more thoroughly. The Government Commission found that the institute buildings satisfy the requirements of scientific work, and with this I quite agree; the institute has indeed already started work.

In equipping the institute, we attempted to make it as perfect and exemplary as possible. I think this aim has been achieved and the institute can be considered as one of the foremost not only in the Soviet Union, but in Europe. When putting up such an institute equipped with all possible conveniences for scientific work, the question often arises both here and abroad, whether the very idea of such an institute is a sound one. For have not the greatest and most remarkable discoveries, nearly without exception, been made with the most simple and elementary apparatus? Why then build such an institute if all the outstanding discoveries have been made with such simple means? Just recently I have been reading J. J. Thomson's autobiographical book *Recollections and Reflections** in which he also discusses this question. His opinion is of particular interest, since of all the physicists of the end of last century and the beginning of the present one it is Thomson whose work has been the most fundamental; it is he who discovered the electron and isotopes, and he used only very simple means. I should like to quote what he says in this connection:

... "It is not, in general, the first discovery of some quite new phenomena that is costly. For example, the discovery of X-rays by Röntgen, of radium by the Curies, the long-continued experiments of C. T. R. Wilson on the formation of drops in particles charged with electricity, cost quite insignificant sums.

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* Published by Bell, London 1936, p. 136.

Discoveries such as these were due to what cannot be bought: to keen powers of observation, to physical insight, to an enthusiasm which does not falter until the difficulties and discrepancies which attend on pioneering work are overcome.

When first the discovery is made, the effects observed are generally very small, and require a succession of lengthy experiments to obtain trustworthy results. It is the attempt to get larger effects that is so expensive. It may mean spending many thousand pounds in making powerful magnets or in producing electromotive forces of hundreds of thousand volts, or in obtaining large supplies of radium. Such money is, however, well spent, for it enables us to obtain new knowledge much more quickly and with greater certainty" ...

I find this view of Thomson perfectly correct. When Columbus set out on the voyage in which he was to discover America, he sailed on a tiny frigate, a mere rowing boat from the modern point of view, but the building of such great liners as the *Lusitania* and the *Titanic* was fully justified since they were necessary for the "rediscovering" of America and for the realization of its potentialities. Thus I think that it is quite correct to follow the line of setting up institutes as perfect as modern technical means can make them.

I should now like to point out some minor details in the organization of our laboratories services. Further there is the system of electrical supply from an accumulator battery, which by means of a control board in the large magnet hall, allows any desired voltage to be obtained in any research room. The leads from the research room go directly to the control board, and if any workers should be simultaneously using the same cells, the potential falls only very slightly, so that the different researches cannot interfere with each other. Then there is the mechanic room: the current from any generator can be supplied to any research room, currents of up to several hundred amperes being controlled by a simple sliding rheostat (which regulates the exciting current of the generator). All the electrical wiring is enclosed in steel and iron pipes to protect it, the whole system being well earthed. There is a good earthing system around the whole of the institute, so that any room can be earthed individually if desired.

Each two research rooms have a photographical dark room between them; provision is also made for darkening every research room. The electric lamps in the room can be connected not only in parallel, but also series, so that the room can be half darkened. Every room has chromium-plated water taps, which allow of fine control of the flow, and gas and compressed air is everywhere available.

The institute has an excellent workshop, where almost any complicated physical apparatus can be constructed. It is equipped with precision lathes, and we can say with pride that the majority of them are of Soviet origin. There, is a separate glass-blower's room, which is spacious and well ventilated, and finally there is a carpenter's shop equipped with all the necessary mechanical devices.

I shall now pass on to questions of the organization of the institute, to which we devoted much attention. Although these questions are not of scientific nature they are nevertheless important for the scientific work. The staff of the institute was very carefully chosen, and even all possible was done to reduce the administrative personnel. When the institute had just started work we kept five accountants. After a good deal of discussion, I was allowed by way of experiment to introduce a simplified system of bookkeeping. The result of this is that at present instead of five accountants we have only one. It will be interesting to see how our experiment works out in future—in any case I think that for such a small unit as our institute this system is worthy of notice. We are also trying an experiment about the fire service. Instead of the previous nine firemen, we have now a voluntary fire brigade and an electric signalling system. By means of such measures as these, we have succeeded in reducing the number of administrative staff considerably below what was orginally planned.

I now come to the most important point, that of the special equipment which our institute obtained from England. We had to decide whether to make this special apparatus here in the Soviet Union or to get it from England; it would of course have been possible to build it here, since our Soviet industry is sufficiently advanced to make anything that can be made abroad, but nevertheless it was found to be extremely difficult to have the equipment made in the Soviet Union. The difficulty is that our industry is not at all adapted for serving the needs of scientific work, everything being done on a large scale, with an almost complete absence of smaller, non-mass-production machine construction. For work such as we scientists require, we have to apply to the instrument shops of factories, and the prices of special individual articles then become enormous, while the time of manufacture is exceedingly great. We scientists should make our industry come to our aid and try to interest it more in our problems.

The special equipment was obtained from England, and for this I must thank my great friend Prof. Lord Rutherford for persuading Cambridge University to sell the equipment of the Magnet Laboratory to the Soviet Union. This apparatus has arrived and is already completely set up here; it consists in the first place of a machine for obtaining strong magnetic fields, which was built as long as 12–14 years ago. This machine is a single phase generator which gives great power on short circuit. It would take me too long to go into all the details of the machine, but the principle on which it is based is quite simple. With an ordinary electromagnet the limit to the strength of the magnetic field obtainable is set by the saturation of the iron, for once the iron is saturated further progress is very slow since the magnetic field increases only as the logarithm of the linear dimensions of the whole magnet. Thus, by constructing a magnet weighing more than 100 tons and costing several million francs, Cotton in Paris was able to gain an increase of only about 25 per cent over what could be obtained with an ordinary laboratory electromagnet. We had therefore to follow another path, and gave up the idea of obtaining steady fields in favour of obtaining only transient fields: this was done by passing a very strong current through a coil, producing an intense magnetic field inside the coil, but for such a short time that the coil had no time to get hot. In Fig. 4 is shown a coil into which we could put some thousands of kilowatts for 1/100 of a second—in this time it warms up to 100°C. If the experiments were to be continued for 1 sec, the coil would heat up to 10,000°C, and would of course melt. In the 1/100 of a sec, we can however already observe all the phenomena which take place in a steady field, because the high intensity of the field makes all the phenomena so much more marked than in the smaller steady fields hitherto used, that they can be observed even in the very short time available. For these observations, of course, special apparatus had to be developed and in England I had already studied a number of phenomena in the strong fields and discovered some new effects.

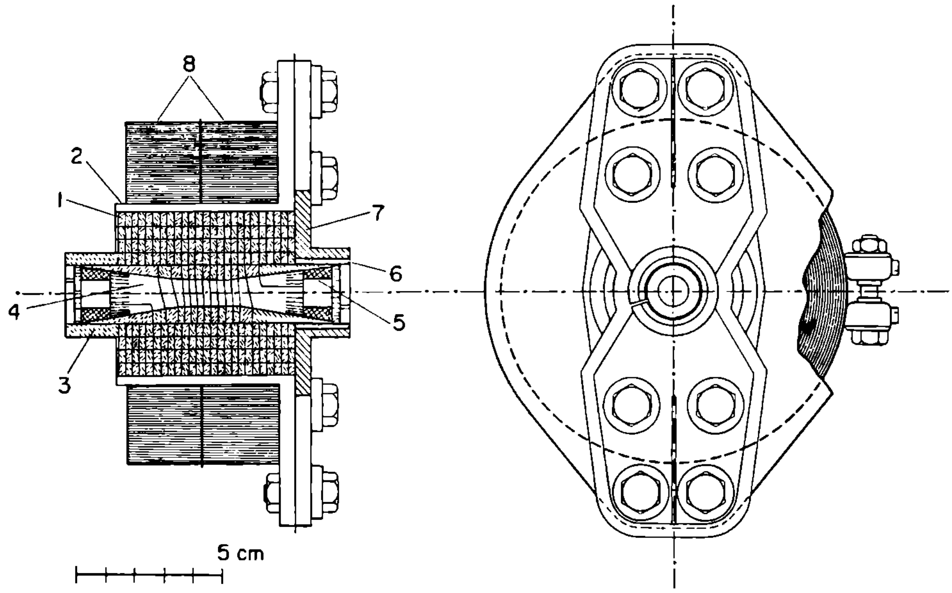

FIG. 4. Coil for obtaining strong fields.

The exterior view of the machine is shown in Plate 6. The rotor weighs $2^1/_2$ tons and rotates at 1500 turns per min; the machine was built by Metropolitan Vickers Ltd., with the collaboration of the Soviet engineer Kostenko and the English engineer Miles Walker. The problem of designing a machine of this type was not simple, since during the time of short current, enormous forces are developed within the machine, which could easily burst it. It had therefore to be made especially strong all; end connections had to be specially fixed, the whole stator made of iron or steel, the bearing made especially strong, and many other details had to make the machine quite individual in its design. On short circuit the machine gave a power of 220,000 kW for 1/100 sec, the current being 72,000 amp at 300 V; this power is colossal, more than a quarter that of the whole of Dneproges. This high power was used only for testing the machine, its actual working power being four times less, so that we have a large safety margin.

The second essential part of the installation is the switch gear, by means of which only a single half period of the current passes through the coil. The wave form of such a half period cut off by the switch is shown in Plate 7.

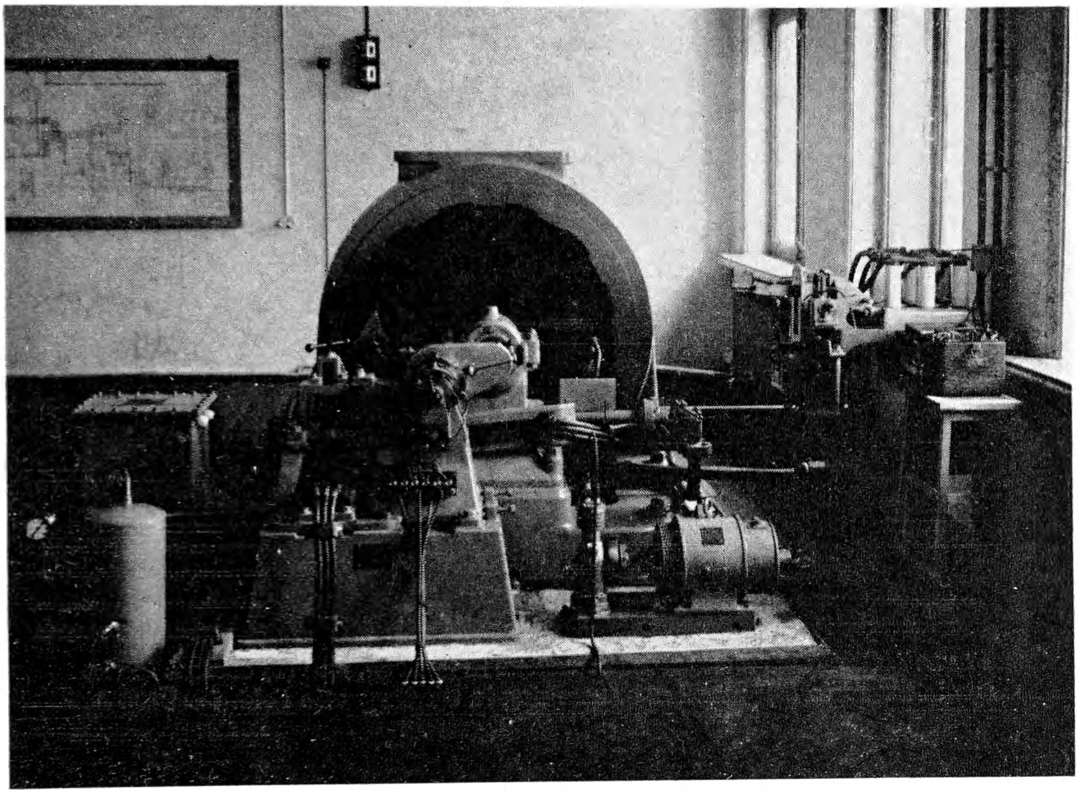

PLATE 6. The generator.

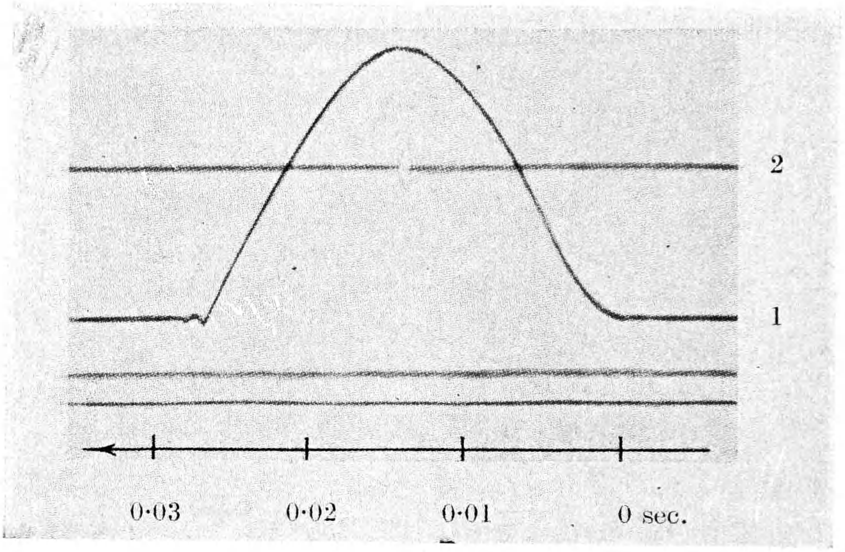

PLATE 7. Oscillogram showing (1) current in the coil, and (2) current in the spark. Break in (2): moment of sparking in the magnetic field. $i = 9400$ A, $H = 277{,}000$ gauss.

One of our greatest difficulties was the design of the coil in which the magnetic field is produced, for when the high current is passed through it, although the coil has no time to heat up, colossal mechanical forces are developed tending to burst the coil. The forces are as high as 150 ton on the cross section of the coil, the pressure on the copper reaching 5000–6000 atm. The first coil tested was in fact torn apart, the diameter increasing from 1 to 2 cm. The circuit broke with great noise, and parts of the coil flew out in all directions. All coil are therefore now fastened down and bound round with a steel bandage. This was not in itself sufficient to solve the problem, however, since the copper still flowed in spite of these precautions; the whole secret of the design of the coil turned out to depend on choosing its shape and winding in such a way that the stresses on the copper became equivalent to a hydrostatic pressure, without any bursting forces. With the help of special calculations made by Cockcroft, it was possible to achieve this, and the coils now work perfectly smoothly.

It is interesting to note that the coil and machine stand at opposite ends of the magnet hall, this arrangement being necessary for the following reason. When the machine is short circuited there is a strong couple breaking the rotor, and since this lasts only for 1/100 sec it gives a strong shock to the foundations, so that something like a small earthquake occurs during the moment in which the experiment takes place. The result of this is that in photographing the Zeeman effect, for instance, the spectrograph vibrates and blurred lines are obtained. It would of course be possible to avoid this by absorbing the shock, but the much simpler method is to separate the coil and machine by a sufficient distance; since the experiment lasts only 1/100 sec the seismic wave reaches the coil only after the experiment is over, when it can no longer produce any undesirable effects. This, then, is the reason for the great length of the magnet hall.

It might seem that the short time of the experiments puts a limit to what we can do, but actually if properly utilized the very shortness of the time is most advantageous. Owing to the high intensity of the magnetic field all phenomena, as I have already pointed out, assume such dimensions that they can usually be studied even in the short time of 1/100 sec—if of course suitable apparatus is used. At the same time all disturbing influences which are time dependent, as for instance thermal disturbances, cease to be appreciable in many cases. As an example I may mention the measurement of magnetostriction of bismuth. During the hundredth of a second that the measurement lasts, the changes of the length of the specimen due to stray thermal disturbances are so small that they do not mask the changes of length produced by the magnetic field, while in steady fields these thermal effects are the principal disturbance limiting the sensitivity of measurement. I could give many more such examples of the advantages of a short time of measurement, but time does not permit of this.

During the course of our experiments with these strong magnetic fields, we found that many phenomena, in particular the galvanomagnetic effects, became most interesting at low temperatures. We therefore turned our attention to the problem of obtaining liquid hydrogen and liquid helium, and began setting up the necessary apparatus. At that time, eight years ago, this was rather difficult as all the experience of the work in this field was available only in the Leiden laboratory of Kamerlingh Onnes, so that a number of practical points of technique, which were very important for obtaining low temperatures, were not generally known. An important part is played in this field by proper organization of work, suitable choice of workers and teaching them to observe a certain standard of cleanliness and proper choice of materials; all these small details being important since their neglect may easily spoil the entire experiment.

I first began by setting up to hydrogen liquefier similar to that which is now working here; the exterior of this liquefier is shown in Plate 8. It differs from the usual type in having a double cycle, one circuit for pure hydrogen and the other for commercial (impure) hydrogen. The cooling is carried out by the pure hydrogen, which cools down and liquefies the commercial hydrogen. In this way it is possible to overcome the most serious difficulty in the liquefaction of hydrogen, since it is unnecessary to use pure hydrogen as the source of the liquid, only the commercial hydrogen being drawn off as liquid. Our liquefier produces 7 l/h with a starting time of 20 min. This short starting time was achieved by proper calculations of all the details of the heat exhanger, and by avoiding the use of any unnecessary metal in the liquefier.

When we constructed the hydrogen liquefier we had in mind the use of liquid hydrogen for the liquefaction of helium, but when we came to consider the liquefaction of helium in detail, we found a new method, by which liquid helium could be obtained without the use of hydrogen. Generally speaking, it is inconvenient to have to work with hydrogen, since it is not safe to use large quantities, and the new helium liquifier which we developed, in contrast to that of Kamerlingh Onnes, permits of helium liquefaction without preliminary cooling by liquid hydrogen.

The idea of the liquefier is very simple. The usual method of helium liquefaction is to cool the helium with hydrogen boiling at reduced pressure until the temperature is reduced below the inversion point of helium; we then have a positive Joule–Thomson effect by means of which the helium can be liquified. The thermal efficiency of such liquefiers is only $^1/_2$–1 per cent. In Leiden liquid helium was produced according to the following plan: on the first day liquid air was made, on the second, 20–30 l of liquid hydrogen, and the liquid helium was made only on the third day, and then only in small quantities.

The only way of avoiding this complicated scheme of operation was to use a reversible process for the cooling—for instance cooling by adiabatic expansion. The problem was thus reduced to design a machine which should remove heat from the helium by expanding it; the design of such a machine involved however a number of difficulties, of which the chief was the design of a piston which would work at low temperatures. The difficulty is that the piston requires lubrication, while at the very low temperatures all substances are solidified. At first we had the idea of using a turbine since this can work without oil, but here we came up against a rather interesting difficulty: helium at very low temperatures has an extremely small specific volume, while the turbine is advantageous only when it passes large volumes—engineers know that a high

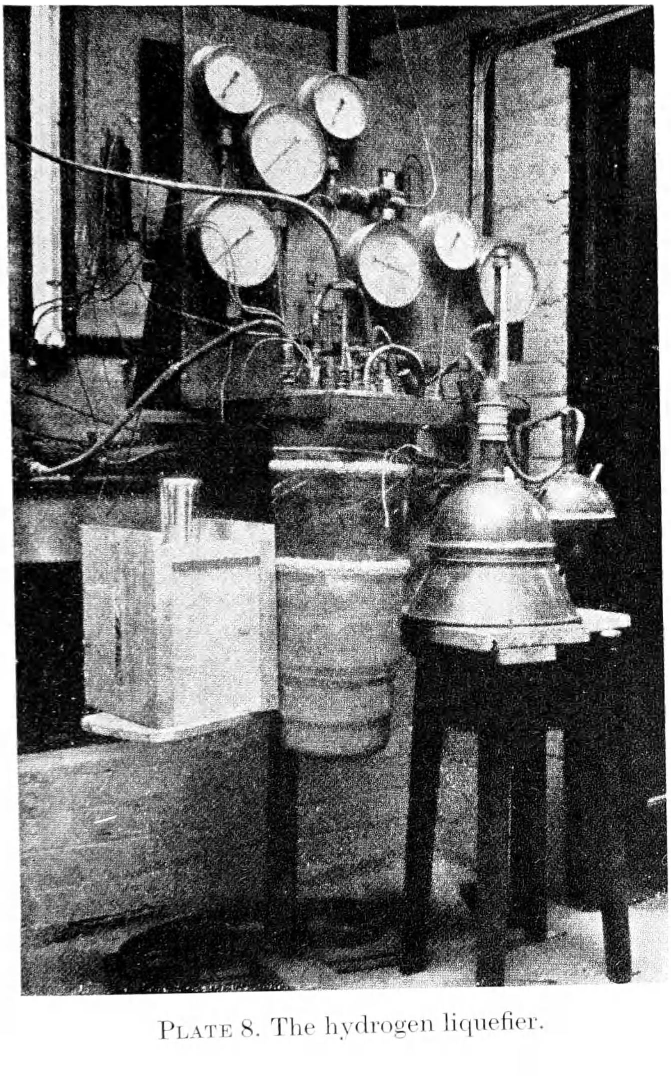

PLATE 8. The hydrogen liquefier.

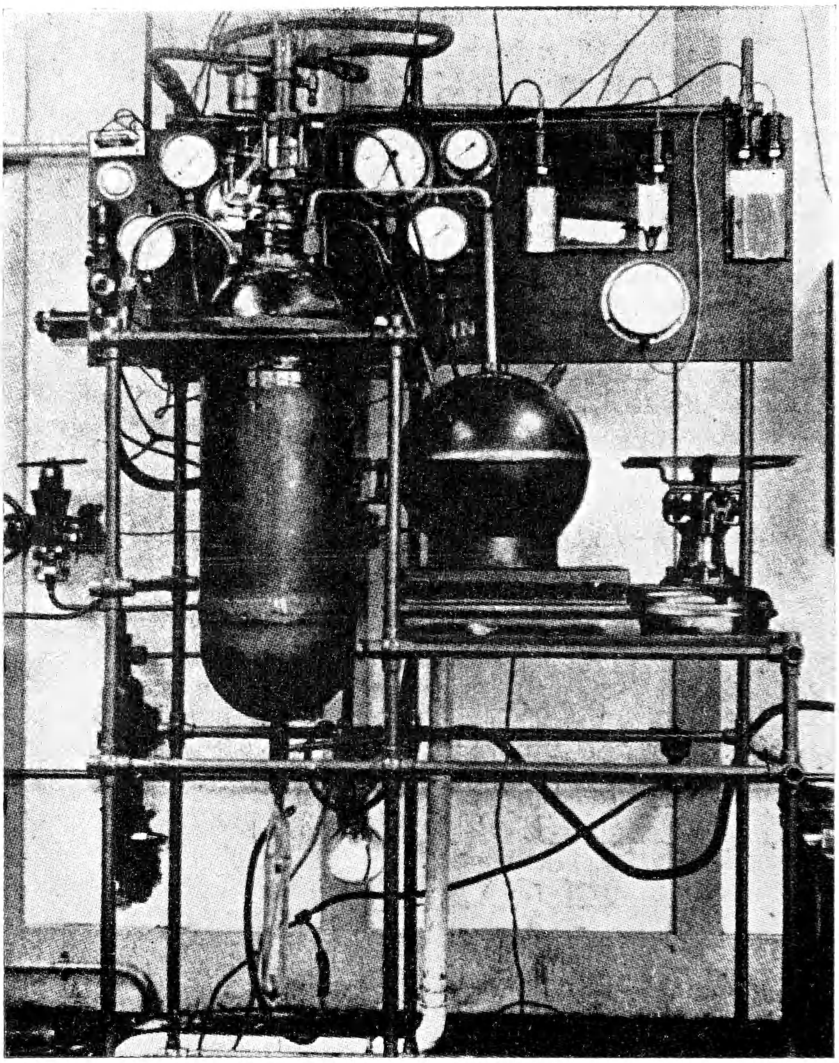

PLATE 9. The helium liquefier.

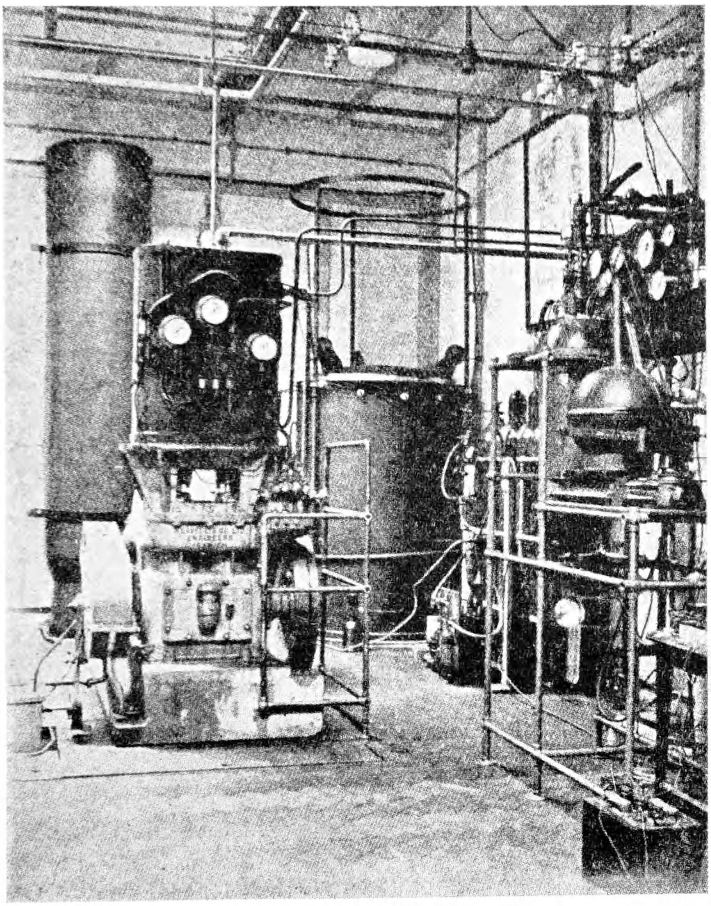

PLATE 10. The helium installation.

efficiency can be obtained with a steam turbine only if it is large. Thus if we consider only turbines of practicable dimensions, they would have to produce no less than some thousands of litres of liquid helium per hour, while for laboratory purposes, where only a few litres per hour are required, the turbine would have to have the impracticably small dimensions of 1–2 cm diameter in order to work efficiently. If very large quantities of liquid helium should ever be required this idea should not be forgotten; for our purposes we had, however, to abandon the idea of a helium turbine, and to work out a machine of the piston- and cylinder type. In this direction we had many different possibilities, for instance a vibrating diaphragm and so on, but the simplest which I was able to think of was the following.

Suppose we have a piston: we cannot make it tight, so suppose we make it quite loose, with a space of some hundredths of a millimetre between it and the cylinder, so that it can move freely. Then when high pressure helium is filled into the cylinder, most of it will naturally escape through the piston–cylinder gap, owing to its low viscosity. If however the piston is allowed to make expansion very rapidly, it is possible to obtain conditions in which only a small fraction of the helium leaks away. The proper working conditions can easily be calculated and it turns out that the speed with which the piston has to move is perfectly possible from a technical point of view. We constructed an expansion machine of this type in Cambridge and it has been working quite successfully up to the present, its thermal efficiency being about 60 per cent.

A difficulty in the construction of the machine was in the choice of a suitable material: most materials become extremely brittle at the temperature of liquid helium, and of course it is not possible to make a machine of a brittle material. The search for a suitable material was not an easy one, but eventually we succeeded in finding one—austenite steel, which remains ductile up to the very lowest temperatures.

The liquefier is shown in Plate 9, the expansion machine being inside the liquefier. We used the following operating cycle, first the helium was cooled to 65°K by liquid nitrogen boiling at reduced pressure, then to 10°K by the expansion machine, and finally liquefied by the Joule–Thomson cooling. This is the first working machine, and it can certainly be considerably improved. It produces 1·7 l of liquid helium per hour. Each litre requiring $1^1/_2$ l of liquid nitrogen. We are now building a machine with a double cycle and hope that it will give 6–8 l/h. The helium installation is shown in Plate 10.

5. LORD RUTHERFORD

THE death of Lord Rutherford is unanimously deplored by all men of science, but especially is it felt by his numerous pupils.

Rutherford's pioneering work, which started forty years ago, has now developed into what we call the science of nuclear physics, and of this science we can call him the creator, since most of the new ideas and discoveries in nuclear physics have been due to Rutherford or to his pupils.

In the history of science, it is difficult to find another case when an individual scientist has had such great influence on the development of science. I think this was mainly possible because Rutherford was not only a great research scientist gifted with exceptional ingenuity, enthusiasm and energy essential for pioneering work, but because he was also a great personality and teacher. His ideas and personality attracted young research students, and his abilities as a teacher helped him to let each of his pupils develop his own character.

His way of dealing with his pupils, whom he called his "boys", was most instructive; when a new research man came to him, Rutherford would first look for any originality and personality in the young man's work. Rutherford would always prefer the man to work on his own ideas rather than to have just another assistant working under his guidance. As soon as Rutherford discovered any sort of originality in his pupil, he would do everything possible to develop it to the utmost; he would encourage him in difficult moments and moments of depression, would not be exigent in case of mistakes, but on the other hand would "put on the brakes" when the young man became too optimistic, drawing premature conclusions from an experiment not thoroughly completed. The pupil of Rutherford would very soon learn that the judgment of his professor was always very reliable and invariably to the point; especially good was Rutherford in judging what ought to be done. Once he told me how Moseley went to him before starting a new problem, and suggested three subjects; Rutherford advised him to choose the work which led Moseley to his famous results on the relations between the wave-length of X-rays emitted by elements and their atomic numbers.

Fairness in acknowledging the originality of the work and ideas of his pupils kept a very healthy spirit in the laboratory, his personal kindness and good will to his pupils gaining the greatest affection that a pupil can have to his teacher. I worked in Rutherford's laboratory for fourteen years, first closely under his direction and later independently, on magnetism, which was rather outside the scope of his line of work, but he continuously took interest in my doings, and to his interest, encouragement and friendship I owe a great deal in the accomplishments of my research.

P. KAPITZA, Lord Rutherford, *Nature*, **140**, 1053 (1937).

Rutherford was fond of his pupils, and to have young research people was indispensable for him, not only because it gave better possibilities of working out a larger number of problems, but also because, as he often used to say, the young students kept him young. This was indeed quite true, for he kept not only young, but if I may say so, even "boyish", to the end of his days. His enthusiasm, energy and gaiety never changed during the years I knew him, and he himself used to say that in research he always felt a young man; he had the same ambition and the same curiosity all through life, and always felt in attacking a new problem that he stood on the same footing as his research men. The young research people helped Rutherford not to age also in another respect, for with his pupils he had to keep up to date in his ideas. He was never in opposition to the new theories, which a large number of physicists of his age would never recognize or else ignore, and I never heard him speak about the "good old age" in physics, when the fundamental laws of Nature were clear and no uncertainty existed.

I cannot think of any country from which young research people did not come at some time to work in his laboratory, in Montreal, Manchester or Cambridge. During my own time in Cambridge, I can remember students working in the Cavendish not only from Great Britain and the Dominions, but also from the United States, Chile, China, Czechoslovakia, Denmark, France, Holland, Germany, India, Italy, Japan, Norway, Poland, the Soviet Union, Switzerland and other countries. Most of them now occupy professorial chairs and some of them have gained an international reputation in science. I am certain that in all these countries there will be men of science who will sincerely mourn Rutherford's death not only as the greatest research physicist since Faraday, but also even more deeply as their teacher and friend.

6. RECOLLECTIONS OF PROFESSOR E. RUTHERFORD

I WAS working with Prof. Rutherford for 14 years, and I remember an episode with which I wish to begin. This was six years ago; a congress was held at Cambridge in memory of the centenary of the birthday of Maxwell—the first director of the Cavendish laboratory, followed by Lord Rayleigh, J. J. Thomson and finally by Rutherford—four great scientists at the close of the past century and the beginning of the present one. After a celebration meeting, where many Maxwell's disciples sharing with us their recollections gave a talk, Rutherford asked me how did I like the talks. I answered:

"The talks were very interesting, but it struck me that all of these spoke exclusively of Maxwell's virtues and represented him as if in the form of a sugary syrup. But I would like to picture myself Maxwell as an actual human being with all of his human features and shortcomings which, surely, exist in any human being no matter what a genius he may be".

Rutherford laughed and charged me to tell after his death the future generation what he was like in reality. Rutherford said this half in joke, and I also laughed.

Well, now, after his premature decease, I am speaking about him and I would like to fullfil this behest. But when I start to picture myself Rutherford such as I would like him to appear to you, then I see that his death and the time which elapsed since our parting have taken away all of his minor human shortcomings. And so a great man of a striking intellect arises before me. I understand now the attitude of Maxwell's disciples who delivered the talks at Cambridge at that time.

In such men as Faraday, Maxwell and Rutherford the exceptional qualities of their intellect and character block out completely minor human imperfections, and when our memory reproduces their picture there remains only their great integrity.

None of us—Rutherford's disciples—expected his death. To us he was not only undying as a scientist but also almost immortal as a human being. Both the father and mother of Rutherford reached their nineties, and Rutherford himself in his 66th year was full of strength, health and energy—he was

П. Л. Капица, Воспоминания о проф. Э. Резерфорде, *Успехи Физических Наук*, **19**, 1 (1938).

П. Л. Капица, *Воспоминания о профессоре Э. РЕЗЕРФОРДЕ*, в *Жизнь для Науки*, Знание, Москва 1965, стр. 35.

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working all day long. However, he fell unexpectedly ill (with strangulation of the hernia) and an operation was necessary; four days after it his organism struggled for life, but his heart failed him, and on the 19th October 1937 Rutherford was gone.

Rutherford was exceptionally popular in his country, all the people knew him, and his death can be considered as a national bereavement. A state funeral was arranged for him, and the urn with his remains was immured in Westminster Abbey together with the graves of Newton, Darwin, Faraday, Herschel and Kelvin.

Rutherford, who departed from life as the greatest scientist of our time, was born in 1871 at the village Brightwater near the provincial town of Nelson in New Zealand. This scientist, who was given all of international distinctions that a man of science could obtain, started his life very unassumingly. He was the fourth child of a small farmer in New Zealand who got subsequently eight more sons. The small farmer cultivating flax was unable to offer education to his twelve children, and so Rutherford from childhood up to the end of his higher education was granted a scholarship.

He was a very vivid, active and gay child interested in hunting and sport. In school and at university he played as a forward in the football team. But he also liked to read, make models, strip mechanisms. Already as a little boy he constructed by himself a camera, which was rather difficult to make at that time. Having finished secondary school he entered Canterbury College at Christchurch. This was a small provincial university at which there were only 150 students and 7 professors. He took great interest in science from the very first day, and began to do research.

In his student days Rutherford was very interested in radiowaves discovered by Hertz. He was fascinated by the idea of wireless telegraphy, but the problem at that time consisted in finding a detector for electric oscillations excited by incident waves. Rutherford discovered that high-frequency oscillations demagnetize iron. In practice this is very easy to observe, if a magnetic needle is put together with a magnetized bundle of iron filaments placed in an oscillating circuit. Then the needle will get considerably deflected as radiowaves are being received. He published this discovery, which produced a fair impression at the small university and at once gave rise to his good reputation.

In 1891 the students organized a small scientific society in which Rutherford already as a very young man read a paper *On the Evolution of Matter*. In this study he put forward ideas completely revolutionary for that time; he stated that all atoms are made of the same constituent parts. This paper was met with disapproval, and he had to apologize to the society. It should be noted that at that time, in 1891, Rutherford disposed of no facts supporting such a statement. It was only in 1896 that radioactivity was discovered, and since the time of Dalton the atom was considered as something unbreakable. But the courage of Rutherford, who suggested such an idea the validity of which he proved experimentally 12 years later, is very instructive.

In 1894 he finished his studies at the university and, having obtained an Exhibition of 1851 Fellowship, he left for England, i.e. for Cambridge. The

"1851 Grant" is the highest one that a young scientist can obtain in England, and it supported completely the scientific work of Rutherford for 2–3 years.

1895 proved to be a year of reforms at Cambridge University. Before this year no graduates from other universities were allowed to work at the Cambridge research laboratories. But owing to an initiative of Prof. J. J. Thomson the order was to be changed, thus enabling graduates of other universities to pursue their scientific work at the Cambridge laboratories.

Rutherford was one among the first young scientists who took advantage of this change. He entered the Cavendish Laboratory whose head was J. J. Thomson. Together with him there entered MacLennan, Townsend and Langevin. In the course of his tenure at the Cavendish Laboratory, Rutherford worked in the same room with Langevin and became his good friend. The friendship of the two scientists who started simultaneously their scientific work was very intimate and indissoluble up to the very death of Rutherford. At Cambridge Rutherford started his work by pursuing his research on radio transmission. He established radiocommunication between the laboratory and the University Observatory, i.e., over a distance of more than two kilometres. He was at that time the first to transmit radio signals over such a large distance. One is bound to think that if he had pursued this work he would have achieved very much, but he was not interested in a practical solution to the problem. At that time he began to take interest in another problem concerning the gas ionization by X-rays whose nature was then unknown. He started to work together with J. J. Thomson, and the two established the phenomenon of the saturation ionization current. This work, published in 1896, can be considered as the fundamental one about this problem.

Just at the time of these researches Becquerel discovered radioactivity in 1896. Rutherford was fascinated by this phenomenon and began to study it. He was the first to show radium to emit radiations of two kinds (he called these α-rays and β-rays), which are distinguished by their ability of penetrating matter. He showed these rays to differ from ordinary rays.

In 1897 Rutherford had become already a young scientist with a certain reputation. In the same year he was invited to hold the chair of physics at McGill University (Montreal) in Canada, and he was working there in the course of ten years (1897–1907). These years, spent at a small provincial university, proved to be the ones of his most fruitful work. It appears to me that this is particularly instructive for young scientists. Indeed, very often one happens to hear young scientists to complain about their being unable to work because there are no favourable conditions, no suitable laboratory, and so on. However, imagine now the young scientist who got far away from his country, completely isolated from the entire scientific world, where at those times even journals came with a delay of many months. But this man was full of ideas and enthusiasm, and in this distant corner of the globe he created the most advanced, revolutionary and leading concepts of the science of that time. Through this he was attracting young scientists from all over the world, and disciples began to come to him.

The work of Rutherford in Canada was marked by a number of the greatest discoveries. Firstly, he discovered thorium emanation. At that time a young chemist named Soddy was working there together with Rutherford, and the two began to study the chemical nature of elements obtained from radioactive disintegration, because it was very important to establish apart from the physical also the chemical properties of the radioactive process. At that time radioactivity was still unelucidated, and Rutherford and Soddy were the first to demonstrate it to be a spontaneous transformation of elements into other ones, which is now called radioactive disintegration. In this case there are emitted either α-rays consisting of rapidly moving positively charged helium atoms, or β-rays which are fast electrons. Starting from this Rutherford assumed thorium emanation to be an element differring from thorium itself. Together with Soddy he determined the atomic weight of thorium emanation on the basis of diffusion and showed it to correspond to an inert gas.

The theory of radioactive decay put forward by Rutherford and Soddy in 1903 produced quite a revolution. When he talked about evolution of matter as a student in 1891 at the student society he had no grounds to corroborate it, but now when he proved it by a number of purely experimental data this produced a colossal impression not only upon the narrow circle of colleagues at his university but also upon scientists the world over. But this concept was all the same so revolutionary that many even very great scientists did not share it. Thus Kelvin died without adopting the idea that radioactivity is the disintegration of the atoms of elements, since he considered atoms to be an unalterable basis of the structure of matter.

The same year Rutherford in his 32nd year was elected a Fellow of the Royal Society (a scientific society equivalent to our Academy of Sciences). But this is no exceptional case in the English academy. There a young scientist is elected as a Fellow as soon as he achieves great successes in scientific work and, therefore, it is not seldom that young scientists 25–28 years old are Fellows. This is a great advantage of the English academy, which makes it an active scientific centre and distinguishes it favourably from academies of all other countries.

In 1907, the physics chair in Manchester became vacant—one of the great universities of Great Britain. In the nineteenth century this chair had been held by such scientists as Dalton, Joule and others. Rutherford moved there. And in the period from 1907 to 1919, while at Manchester, he did a whole series of works no less great than in Montreal. Of his works in this period one should mention, first, the work on the scattering of α-particles in passing through matter. It led to Rutherford fixing on a new model of the atom, generally accepted to this day.

In 1908 he received for his work the Nobel prize in chemistry. In 1919 he discovered artificial disintegration of matter and showed that in Nature not only spontaneous decay of radioactive elements exists, but one can cause also artificial decomposition of the nucleus under the influence of bombardment by α-rays. This was discovered on nitrogen, and then verified on a number of other light elements. Thus he created an entirely new field of nuclear physics—artificial nuclear disintegration.

Just as in Canada, so in Manchester, he attracted a whole "pléiade" of young scientists. With him worked not only Englishmen, but also the German Geiger, the Dane Bohr and others, and in his laboratory his pupils did a series of outstanding works.

In 1919 Rutherford got the chair at Cambridge, he went there and spent the rest of his life at Cambridge as director of the Cavendish laboratory, a post vacated by J. J. Thomson upon his retirement. Here he continued his work on artificial disintegration of matter. He guided the research of his pupils, and in his laboratory they made two of the greatest discoveries of nuclear physics in the last decade—the discovery of the neutron by Chadwick and the work of Cockcroft and Walton on artificial disintegration of matter under bombardment by a beam of protons produced artificially.

We see that beginning his experimental works on radioactivity in 1896, Rutherford developed them unswervingly and by the end of his life this field of knowledge assumed such dimensions that it presents itself to us already as a separate science—nuclear physics.

To appreciate the value of each discovery of Rutherford, one has to visualize the historical background against which they took place. This task is far too broad for an article such as mine. But it is very instructive to trace on individual examples the methods by which Rutherford conducted his scientific work and by which he achieved such great results.

Rutherford was an experimentalist and in this respect he is reminiscent of Faraday. He used few formulae and resorted not much to mathematics. Once, trying to derive a formula for his lectures, he got confused and so simply wrote the result, remarking "If all is carried out correctly, then it will be obtained so". One may say that he "saw" the phenomenon on which he was working, though it took its course in the immeasurably small nucleus of the atom.

If one may speak very schematically, amongst physicists there are as it were two types of investigators. Some—this is the type rather of the German school, where the experimentor proceeds from known theoretical propositions and tries to verify them by experiment. But the other type of scientist, rather of the British school, proceeds not from theory, but from the phenomenon itself—he studies it and looks to see whether the phenomenon can be explained by existing theories. Here the studying of the phenomenon, its analysis, is the main motive for the experiment. And if such a division is possible, Rutherford was a striking example of the second approach in experimental physics. The main thing for Rutherford was to inquire and to understand the phenomenon. Experiments must be so constructed that what the phenomenon consists of becomes clear. Accordingly the accuracy and complexity of measurements ought to be just as required to inquire into the phenomenon and understand it.

As an example, I will present the case of α-particle radiation. Radium emits α-particles. Rutherford showed these rays in the very beginning of his experiment to be unusual radiations. But what were these particles?

Rutherford concluded that, since they were ejected from radium, they must represent some already existing element. In order to find out what is this element, it was necessary only to determine the mass, and only to an accuracy such that one might see which existing element corresponds to the mass.

Rutherford set up an experiment which is very characteristic of him. I will describe this experiment, although it is only of historical interest, since at present more accurate and complex methods are used for the determination of the mass of α-particles. But the initial method of Rutherford is striking by its simplicity and by its leading straight to the goal.

Figure 1 shows the arrangement for these experiments. The simple electroscope *Q* made of a gold foil is placed above 20 metal plates *A* set in parallel. The gap between the plates amounts to only 1 mm so that α-rays emitted by the radioactive salt *B* (put at the bottom) may come in a parallel beam to the chamber of the electroscope. In order to remove the emanation and increase the path of α-rays hydrogen was let pass through the device.

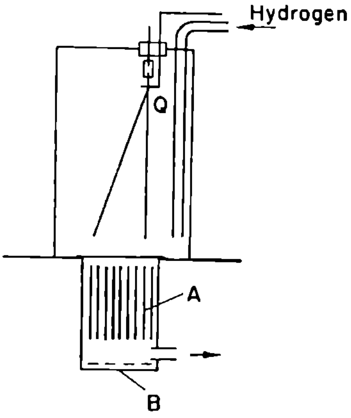

FIG. 1. Instrument for determining the composition of α-particles.

Applying a strong magnetic field in parallel to the plane of the plates *A* the ionization in the chamber of the electroscope could be almost completely discontinued, In such a simple way Rutherford showed α-rays to be rapidly moving charged particles. By screening at the side of the electroscope one half of the gaps between the plates it could be shown that for one direction of the magnetic field the ionization was stopped at a lower field strength than for another. Thus the direction of deflection of α-rays by magnetic field was established and hence it was inferred that the charge of α-particles is positive. Producing an electric field between the plates *A* by connecting them in turn to the opposite poles of a battery, Rutherford succeeded in discontinuing the ionization and in deflecting the α-rays by the electric field. From these data he determined the velocity of the α-particles and also showed them to be a flux of positively charged atoms of a mass larger than that of hydrogen atoms, and determined their charge to mass ratio with an accuracy to within 10 per cent. This ratio pointed to the fact that α-particles correspond to doubly charged helium atoms.

But it was necessary to prove more accurately that these were actually helium atoms. This work was undertaken later (in 1909) at Manchester where he disposed already of large stocks of radium.

The device for these experiments was also extremely simple. It is shown in Fig. 2. In the small thin-walled glass tube *A* there was put radon. The thickness of the walls of this tube amounted only to 0·01 mm, so that fast α-rays could pass through the glass whereas the emanation was preserved. This tube was placed in the glass vessel *B* having at its end a discharge capillary with the electrodes *C* and *D*. Vacuum in the space around the tube *A* was produced by raising and lowering the mercury in the vessel *B*. The tube with the emanation remained in the device during two days, and thereupon the gas produced by the passing α-rays was compressed by raising the mercury into the discharge tube.

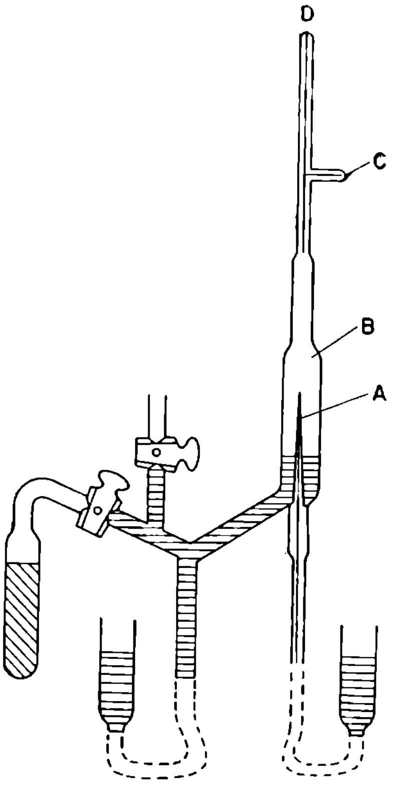

FIG. 2. Experiment confirming that α-particles are helium atoms.

In the luminescence of the tube there were seen yellow helium lines which proved the presence of helium. By means of a control experiment in which this tube was filled by helium it was easily shown that the helium did not diffuse out of the tube with the emanation. In this case the helium lines did not appear in the spectrum. Thus α-rays were shown to be actually helium atoms.

The two experiments that I described are exceptionally simple, so that any student may carry them out. But at the same time these experiments, so properly set up and so directly leading to the goal, solved at that period a problem of paramount importance and produced a revolution in concepts on matter.

Rutherford was not satisfied by the investigation of merely α-ray beams by means of observing the ionization produced by them, and searched for a method by which he would be able to detect individual α-particles. The first such method was found in observing scintillations.

Already Crookes noted that under the action of a bombardment by positive rays certain substances became luminescent. The most intensely luminescent substance proved to be zinc blende. When Rutherford together with Geiger put zinc blende under a microscope and let α-ray beams impinge on it, then instead of seeing in the micrsocope field of vision a homogeneous luminescent background they saw individual scintillations. They concluded that the scintillations arose at spots where α-particles hit the zinc blende. Thus the number of α-particles emitted could be determined from that of the scintillations produced on the zinc blende.

Another method of observing α-rays discovered by Rutherford, i.e. the method of counters, owing to the invention of amplifier tubes became now even more powerful than that of counting scintillations. This method is based on a phenomenon discovered by Townsend. If in a gas at a low pressure there is a spike, then one can choose a potential such that a discharge just does not arise. If now even the weakest ionization by even a single α-particle is produced in the gas, then a discharge will immediately take place for a certain time interval. In 1908 Rutherford and Geiger constructed the first counter operating on this principle. It is shown in Fig. 3. Instead of a spike the fine filament A was put in the cylindric vessel B. The critical potential was produced between the filament and the cylinder. Alpha-rays, whose source was in the vessel D, could penetrate through the opening C covered by a very fine mica foil. The discharge current from the filament was registered by a wire galvanometer; the number of α-particles could be determined from deflections of the galvanometer. Nowadays the wire galvanometer in the counter invented by Rutherford and Geiger is replaced by an amplifier tube, which makes the counter extremely sensitive. In its contemporary form, the counter represents one of the most essential devices due to which a complete investigation of cosmic rays was made possible.

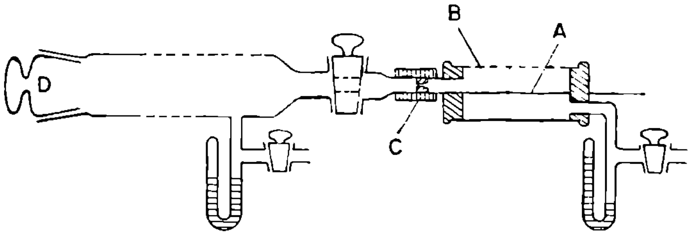

FIG. 3. First gas-discharge counter for charged particles, constructed by Rutherford and Geiger in 1908.

Having a possibility to count α-particles Rutherford began to study a number of phenomena which earlier did not lend themselves to an investigation.

In 1910 a young scientist called Marsden came to Rutherford to work at the laboratory. He asked Rutherford to give him some very simple problem. Rutherford instructed him to count α-particles passing through matter and to find their scattering. In doing this Rutherford noted that in his opinion Marsden would discover nothing outstanding. Rutherford's concepts were based on the Thomson atomic model adopted at that time. In terms of this model the atom appeared to be a sphere of a diameter of 10^{-8} cm with a uniformly distributed positive charge interspersed with electrons. The harmonic oscillations of these last determined the emission spectra. It can readily be shown that an easy passage of α-particles through such a sphere and no marked scattering were to be expected. The overall energy of α-particles was during their travel consumed to eject electrons which ionized the neighbouring atoms.

Marsden guided by Geiger began to carry out his observations and soon noted that the majority of α-particles passed through matter but that there was nevertheless an appreciable scattering while some particles were recoiled back. When Rutherford was informed about this, he said:

"This is impossible. This is just as impossible as a recoil of a bullet from paper".

This sentence shows how concretely and pictorially he imagined the phenomenon. Marsden and Geiger published their work, and Rutherford at once concluded that the existing concept of the atom was inaccurate and that it needed a radical revision.

Studying the law of distribution of scattered α-particles, Rutherford tried to find out what field distribution inside the atom was necessary in order to define the law of scattering at which the α-particles might even be scattered backward. He came to the conclusion that this last phenomenon is possible if the entire charge is concentrated in the centre and not throughout the entire volume of the atom. The size of this centre, which he called the nucleus, is very small: the diameter is 10^{-12}–10^{-13} cm. But where were the electrons to be placed? Rutherford concluded that negatively charged electrons must be distributed around the nucleus; they can stay there owing to their rotation around the nucleus, when the centrifugal force balances the attractive force of the positive nuclear charge. Consequently, the model of the atom appears to be none other than a sort of solar system consisting of a nucleus—sun and electrons—planets. Thus he established his model of the atom.

This model was met with utter bewilderment, because it contradicted certain basic physical concepts of that time which were thought to be unalterable. Of course, Rutherford knew that on the basis of the Maxwell theory the electrons rotating around the centre must inevitably emit light, lose their kinetic energy and sooner or later fall on the nucleus. At those times it was very difficult to contradict the bases of the Maxwell theory. Therefore the Rutherford atomic model was at first not adopted.

The situation was the same for two years. Then a young Danish scientist Niels Bohr came to Rutherford to work. They often discussed this atomic model. To Bohr it was also clear that the principles of this model disagreed with the laws which were at that time considered as fundamental ones. And so Bohr began to study this paradox. He trusted the experimental basis of the Rutherford model, but it was neccassary to substantiate it theoretically. The brillant idea struck him that the basic conceptions of the quantum theory of radiation which just appeared might be used to provide the theoretical substantiation. These concepts were first put forward by Planck and then considerably generalized by Einstein.

In 1913, Bohr gave the theoretical grounds of the Rutherford atomic model, which is now called the Bohr–Rutherford model and on which all contemporary atomic physics is based.

One of the basic traits of Rutherford in his experimental work was his exceptional keenness of observation, ability of generalizing a phenomenon and of bringing to light what was most important and essential. This can be seen in a number of instances. For example, when he discovered thorium-emanation he started from the observed difference in ionization produced by thorium when the electroscope chamber was open and closed. It seemed that the air flow passing through the preparation changed the radioactivity of the thorium itself. Rutherford gathered this air and discovered at once that it was also radioactive. This was just the discovery of the emanation. Most scientists upon seeing the difference would start to investigate the phenomenon either with a closed or with an open electroscope chamber. But Rutherford raised at once the question why this phenomenon was just such, and tried immediately to bring to light its essence. It was this necessarily arising question "why" that was the clue to great discoveries.

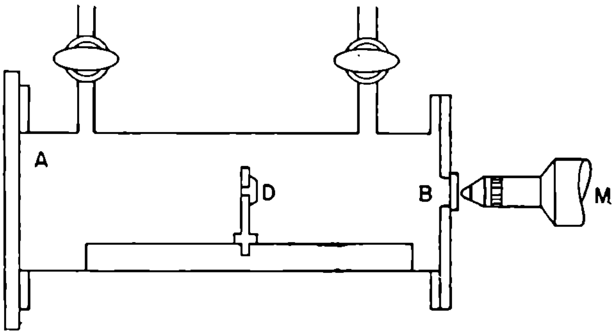

FIG. 4. Instrument by which Rutherford effected artificial disintegration of elements (nuclear reactions).

And here is another case. His remarkable power of observation manifested itself also in discovering the artificial disintegration of matter. The fact is that in observing scintillations it often turned out that rays of a very long range—much longer than that of the bombarding α-particles—were emitted by the bombarded substance. Everybody observed these rays and frequently talked about them, but nobody tried to explain them, nobody put to himself the question "why". Rutherford concluded that this phenomenon should be analysed and that an attempt should be made to elucidate it. An explanation was soon found. It turned out that under the action of α-ray bombardment the nitrogen atoms which are always present in air underwent a decay. It was this that accounted for the long range. Rutherford set up his experiments exceptionally simply. Figure 4 shows his device.

The hermetically sealed chamber A can be filled by gas at different pressures through two taps. D is a source of α-particles, B is the screen on which the scintillations are observed by means of the microscope M. The screen is at the side of the α-ray source covered by a silver plate which absorbs a considerable part of the energy of their travel. Filling the chamber A by nitrogen, Rutherford noted that at a certain pressure most scintillations vanished. This took place when α-rays emitted by the radioactive source spent their entire energy to ionize the air and could not reach the screen. But the remaining scintillations pointed to the presence of a very small number of α-particles of a range several times larger than that of particles emitted by the source. When instead of nitrogen another gas was used, then such residual scintillations did not occur. The sole explanation was that they were due to nitrogen. Since the energy of the residual α-rays was larger than that of the primary ones, the former could arise only from a decay of the nuclei of nitrogen atoms. Thus the decay of nitrogen was proved, and the problem of alchemy was solved in principle.

Such a simplicity in stating the problem which was given such a simple experimental form cannot but strike any investigator, not only a physicist.

Many people say that Rutherford possesed an exceptional intuition—as if he felt how an experiment was to be carried out and what was to be searched for. Intuition is usually meant to be some unconscious process—this is something that cannot be explained and which leads subconsciously to the right solution. I personally think that this may in part be true, but that at any rate this is grossly oversimplified. An ordinary reader has not the slightest idea about how hard every scientist works. A layman gets to know only that part of scientific work which leads to definite results. Observing Rutherford closely, one could see what an enormous amount of work he was doing. His energy and enthusiasm were inexhaustible. He was working and searching for something new all the time. He published and let his colleagues only know of studies that led to definite results, but such studies constituted hardly more than a small percentage of the enormous amount of work he was doing; the rest was not only unpublished but even remained unknown to his disciples. Sometimes only from casual hints could it be guessed that he tried to do something but that nothing came of it. He did not like to speak about his research projects and rather spoke only of what was already performed and yielded results.

One of the most striking examples of his exceptional perspicacity is the discovery of the neutron. The neutron is a material particle of the same mass as the hydrogen nucleus, but uncharged. Experimental evidence for the existence of such a particle was provided by Chadwick—Rutherford's closest disciple—at Cambridge in 1932. For this discovery Chadwick obtained the Nobel prize. He studied a phenomenon in which extremely penetrating rays were obtained as a result of bombardment of berillium by polonium γ-rays. He succeeded in showing these to be no γ-rays. This radiation was first detected by Bothe and subsequently investigated by the couple Joliot-Curie, but only Chadwick succeeded in interpreting it; he proved this radiation to be neutrons. The discovery of the neutron plays a very important part in contemporary nuclear physics, because the neutron is one of the basic elementary particles constituting the nuclei of all elements.

It turns out that Rutherford—twelve years before the discovery of the neutron—predicted in great detail the possibility of its existence. Below is given an excerpt from a lecture of Rutherford read at the Royal Society in 1920.

"If we are correct in this assumption it seems very likely that one electron can also bind two H-nuclei and possibly also one H-nucleus. In the one case, this entails the possible existence of an atom of mass nearly 2, carrying one charge, which is to be regarded as an isotope of hydrogen. In the other case, it involves the idea of the possible existence of an atom of mass 1 which has zero nucleus charge. Such an atomic structure seems by no means impossible. On present views, the neutral hydrogen atom is regarded as a nucleus of unit charge with an electron attached at a distance, and the spectrum of hydrogen is ascribed to the movements of this distant electron, Under some conditions, however, it may be possible for an electron to combine much more closely with the H-nucleus, forming a kind of neutral doublet. Such an atom would have very novel properties. Its external field would be practically zero, except very close to the nucleus, and in consequence it should be able to move freely through matter. Its presence would probably be difficult to detect by the spectroscope, and it may be impossible to contain it in a sealed vessel. On the other hand, it should enter readily the structure of atoms, and may either unite with the nucleus or be disintegrated by its intense field, resulting possibly in the escape of a charged H atom or an electron or both."

Thus Rutherford predicted long in advance all of the basic moments determining the trend in the development of the whole of nuclear physics after the discoveries of Chadwick and Joliot-Curie.

I would not call this process intuition. This was the process of profound thinking and thorough experimental work. All of us know that Rutherford was searching for the neutron—he searched for it for a long time and persistently, but did not find it where he expected it. In this case a great deal depended on chance. One could not foresee theoretically why just beryllium and polonium and not other substances were to be chosen for the study. There one had simply to search persistently.

Being such an exceptionally great scientist, Rutherford was also an outstanding man in this respect that he did not confine himself to the circle of his laboratory, but was widely interested in the whole world.

In his appearance he was rather solid, of a medium stature, with blue always gay eyes and a very expressive face. He was agile, used to speake fairly loudly and could not speak in a low voice. When the professor was coming to the laboratory everybody got to know this, and from the intonation of his voice it could be guessed whether or not he was in good spirits.

Already from the first word a frankness was evident in the whole of his manner of intercourse with people. His answers were always brief, clear and accurate. Through his affability he attracted people rapidly. It was very agreeable to keep his company. When he was to ldsomething he reacted at once, no matter what it was. One could discuss any problem with him—he started readily to consider it. I remember, for instance, that one day after dinner, when several scientists (including Eddington) were sitting talking over a glass of port, the problem of the meteorite that fell in Siberia was discussed. This meteorite was just then discovered, and we were very interested in it. The problem was considered in detail, and we calculated roughly the energy and size of the meteorite from the data we disposed of. One of us put the question:

"What is the probability for such a meteorite to fall on the City of London, i.e. there where all London banks are concentrated?"

We calculated the probability, and it turned out to be very low. There were also some economists, and so the following question was also put:

"What would be the effect of a destruction of the City—i.e. of the London bank machinery—on the English State, if all the industry remains?" In this discussion everybody put forward his assumptions, and the conversation lasted for two hours. Rutherford used to take an active part in discussions of such a kind.

When anybody made a witty remark or conclusion, Rutherford was the first to laugh, and positively more loudly than all the others. If he was taken with something, he made at once a comment. He was not at all hypocritical, nor did he keep his feelings to himself. When he aptly played football he was pleased. He also rejoiced at a successful experiment, and was happy when he was given a scientific reward. When he was angry he also did not dissemble it. But he was also an exceptionally good man, easy to appease. I never saw him to be angry for more than five or ten minutes; thereupon his anger was completely over.

Rutherford treated everybody very carefully, in particular his disciples. When I came to him to work in the laboratory, I was immediately struck by this care. He did not allow us to work in the laboratory after 6 p.m., while on rest-days no work whatever was permitted by him. I protested, but he said:

"It is completely sufficient to work till 6 o'clock; the remaining time you should think. Those who work too much and think too little are poor ones."

If he noticed that I was tired, he told me immediately: "You should go away and have a rest." I objected: "Please, it is not as yet the time for holidays." He replied in jest: "Never mind, I shall lock your room and will not allow you to work."

I saw afterwards that the time spent for relaxation was completely compensated by the energy acquired.

From the picture of Rutherford that I depicted you will see at once a man full of temperament. This temperament was manifested in everything, particularly in his judgements. Rutherford could not stand any careless work and unfair competition. When any of his disciples manifested even the slightest lack of conscience in anything—be it by an incorrect representation of their results or by not quoting the source of their ideas and so by attempting to represent their work as an original whereas in fact the idea of the work was taken from elsewhere,—Rutherford lost any interest in such men.

Rutherford himself was extremely accurate in giving credit where credit was due. When any of his disciples carried out their work on their own idea, he noted in particular that the work was performed on the idea of the disciple. When a research was carried out on his idea he also mentioned this. He found it necessary to be correct.

As an example, I will quote the story of a great discovery made by his disciple Moseley, who worked with Rutherford in 1912. Moseley was then very young, but Rutherford always quoted him as his best disciple. Soon after he came to work in the laboratory Moseley performed several brillant pieces of research. Thereupon he proposed to Rutherford the themes of three studies he wanted to perform, and described these in detail. One of these studies was the striking work that made him famous all over the world—the establishment of the dependence of the wavelength of X-rays of the atom on the position of the atom in the periodic system. Rutherford said to him that in his opinion the study was of great importance, and suggested to Moseley to carry out just this one. Rutherford was not mistaken—the work turned out to be indeed of extreme importance. But the idea came from Moseley, and Rutherford always noted this (Moseley was killed in battle during thc First World War when he was 27 years old).

With such a character Rutherford was an exceptional teacher, and established a great school. Rutherford was a teacher rather than a pedagogue. When in 1894 after having finished his university studies he was made a secondary-school teacher, this year of his pedagogical activity proved to be very unsuccessful. In the class there was always noise and disorder, and when he was explaining something none of the pupils understood it. The pupils soon got to know that when he was angry and put a pupil out of the class this was not to be much afraid of, because he was soon carried away by his talk and the pupil could imperceptibly steal back to the class. During his talk he did not care about anything.

He was unable to establish school discipline. Sometimes, when in his childhood his mother charged him to take care of his little sisters and to read them something the sisters used to run away. And so, in order to prevent them from running away, he simply tied together their plaits.

He read his lectures in a very interesting manner, but not interesting for all of the students. For example, when he was reading a lecture on the gas kinetic theory and approaching the problem of molecular collisions—he recalled α-particle collisions and started to talk about his last experiments—and the lecture took a completely different sense. But he talked in an extremely captivating way about the things that interested him. The students which were in need of knowledge necessary for practical purposes—to pass examinations—did not approve much of the Rutherford way of reading lectures on physics; they preferred to be given lectures according to the programme.

To be sure, a man full of such an enormous enthusiasm, of such a deep understanding of the subject, and of such a character could not but be a great teacher. He carried his students with him and this showed up. When he was still quite young and working at Montreal, Canada, scientists from all over the world started already to come down to him to work, for example, the well-known German chemist Hahn, and many others. At that time Rutherford was not even 30 years old. When he moved to Manchester, he got surrounded by quite a number of disciples. Among these there were Bohr, Geiger, Marsden, Moseley, Darwin, Chadwick, Robertson and many other scientists whose names are not so famous as the ones listed. At Cambridge he had as before a lot of disciples, and many of these became excellent scientists. Of these one can quote Cockcroft, Oliphant, Blackett, Chadwick, Ellis, Henderson and many others. With regard to his disciples Rutherford was very careful. His outlook on his disciples was schematically the following. He used to say:

"If a young disciple has been working with me and after two years of work comes to me with a question what he has to do further, I advise him to give up any further work in the field of science, for if a man after two years of work does not know what to do, then no scientist will become of him."

In fact he never treated the thing so sharply, and always under various pretexts found a possibility to provide his unsuccessful disciples with a post at an industrial laboratory or with a post as teacher at a secondary school or university.

But when a disciple manifested initiative and individuality, then Rutherford gave such a man his full support and attention. It should also be mentioned that Rutherford tried his best to reveal the individuality of any disciple. I recall that in the beginning of my work at Cambridge I came one day to Rutherford and told him:

"X is working with you, but he works on a hopeless idea and is wasting time, facilities and so on."

"I know this," Rutherford replied. "I know that he is working on an absolutely hopeless problem, but this problem is the one he stated, and if nothing comes of the work, this will teach him to think on his own and lead him to another problem that will this time not be a hopeless one."

He was ready to try anything in order to foster independence and originality of thought in his disciples. When a disciple started to make progress and tried to think originally, Rutherford took every possible care of him and encouraged him to work. This highly correct attitude was manifested also in that he always acknowledged the ideas of his disciples. He saw to it that everything that pertained to somebody be noted. He himself always noted this in his lectures. If anybody in publishing his work forgot to mention that the given idea was not his own, Rutherford immediately objected. He saw in every possible way to it that complete fairness and true priority be maintained.

Rutherford was completely aware of the importance of his disciples for him. For him the point was not only in raising the scientific efficiency of the laboratory. He used to say: "The disciples make me remain young."

There is a profound truth in this statement, because disciples do not allow their teacher to lag behind life and fail to keep pace with new scientific facts. We often note that many scientists as they grow old oppose new theories and underestimate new trends in science. However, Rutherford adopted readily and benevolently new ideas of physics, such as wave and quantum mechanics, which are still groundlessly regarded with reserve by a number of great scientists of his generation. This last usually happens with those scientists who have no close disciples to guide any further.

From all said here it appears clearly that the death of Prof. Rutherford is a very hard blow for scientists the world over. Science lost by him the greatest pioneer in physical research since the time of Faraday. In the course of his entire life, so fruitful in scientific discoveries, he was working on the most fundamental problems of contemporary atomic theory.

He can be considered not only as the founder of a new chapter in science, but also as the founder of quite a new science—nuclear physics.

Already in 1896 he began as a very young man to study radioactivity which was just then discovered, and since that time his research which he carried out during 40 years yielded every year to humanity new discoveries and new ideas which have been the leading ones in atomic physics the world over.

His influence on international science got considerably increased owing to a large number of disciples of different nationalities, including a number of Soviet scientists, who worked in Rutherford's laboratory. For their part Rutherford's selfless and great personality deserved not only esteem and admiration but also a profound love. Thus the greatest school of physicists that ever existed was created around him. And thus it is quite understandable why his death is felt by such a large number of scientists the world over as a great personal loss and grief.

His death is felt with a particular sadness here in the Soviet Union where science is recognized as one of the fundamental bases of social development.

7. LIQUID AIR

TALKING about a new apparatus for the production of liquid air, which we worked out at the Institute of Physical Problems, I wish to dwell upon what called forth this work and upon theoretical considerations that underlay its development.

The scientists Cailletet and Pictet were the first to liquefy air in 1877. But liquid air was in the beginning of no practical importance. Its introduction was hindered by the extreme complexity of the apparatus designed by Pictet. It was only in 1895, when Linde and Hampson almost simultaneously worked out a simpler apparatus for the production of liquid air, that it began to take root in laboratory practice. In spite of the fact that the Linde apparatus was of low efficiency, owing to its simplicity it was widely used. At one time almost every large laboratory had a Linde apparatus. Here and there these devices have been preserved and are working till now.

Investigation of the properties of liquid air led to a remarkable discovery, made almost simultaneously (in 1899) by Linde and Baly. They observed that nitrogen boils at a temperature lower by 12·8°C than the boiling point of oxygen. Hence when liquid air, which, as is known, contains both nitrogen and oxygen, is being evaporated, it is nitrogen that evaporates from the very beginning at a higher rate, while the residue is being enriched by oxygen. Linde was the first to draw attention to the fact that this property of liquid air can be used for producing pure oxygen by means of multiple distillations like, for example, in the oil-industry and in other cases where it is necessary to fractionate two liquids having different boiling points. In the beginning of the last century Linde began to design a number of devices for producing liquid and gaseous oxygen.

As soon as low-cost oxygen was available, one began at once to apply it in technology and mainly in autogenous work. The demand for oxygen increased ever more with increasing autogenous work and, hence, plants of an ever higher capacity where being built for the production of gaseous and liquid oxygen.

When oxygen became available on a large scale, industry began to apply it also in other branches of the national economy. There grew up important branches of industry using oxygen. But the development of a number of important branches of industry depends on a cheaper oxygen production. The high demand of industry for oxygen is accounted for by the fact that oxygen is associated with the process of combustion, which is the basic one in a number of very important factories.

П. Капица, Жидкий воздух, *Техника Молодежи*, 3, 28 (1939).

П. Капица, Проблема жидкого воздуха, *Плановое Хозяйство*, 2, 73 (1939).

Text of a report presented at a meeting of the Presidium of the Academy of Sciences of the U.S.S.R. on 25 December 1938.

Air contains, beside oxygen which is directly necessary for the combustion of fuels, four times more inert nitrogen. The latter not only lowers the combustion temperature but also gives rise to heat losses (leaving the furnace, nitrogen carries away heat). Calculations show that in many cases even a partial enrichment of air with oxygen can increase considerably the intensity and economy of thermo-technological processes. That is why there is such a high demand for oxygen-enriched air on the part of metallurgy, energy economy and gasification of coal and mineral oil. Ten thousand or so cubic metres of oxygen per hour are required by our industry.

Nitrogen separated from oxygen, which in the form of nitrogenous fertilizers is the basic product to satisfy the vital need of our agriculture, is demanded in no less amounts.

Finally, when the methods of fractionating air get improved, it will be possible to isolate from air rare gases, contained in it in very negligible proportions. Of these one should mention in particular krypton and xenon, which are the best fillers for filament lamps. The efficiency of electric lamps increases by 20–30 per cent if they are filled with krypton or xenon.

Such a high demand from industry for the gases contained in air induces scientists to search for the most economical methods of producing these gases.

Thus, in order to obtain oxygen and nitrogen from air, these gases must be fractionated. It can be calculated theoretically that at least 0·068 kWh should be spent for the isolation of one cubic metre of oxygen from air.

But this minimum refers to expenditures in extracting oxygen from air. Can oxygen be obtained with lower expenditures from some other sources? Science answers in the negative. Oxygen is present in the free state in air, whereas in other substances it iscontained in a bound state, hence theoretically the production of oxygen from air is the most economical.

As a matter of fact, even in the best foreign plants with a large-scale production, 0·5 kWh are spent for the production of one cubic metre of oxygen, i.e., eight to nine times more than it would be necessary according to theoretical calculations. Consequently, the efficiency of these plants does not exceed 14 per cent.

Why are all these cycles of such a low efficiency? What is the matter here? Which stages of the process of fractionation are wasting such a tremendous amount of energy?

The fractionation of oxygen and nitrogen must be carried out at a very low temperature, −194°C. At this temperature, which differs very much from room temperature, penetration of heat into the apparatus is hard to prevent. Indeed, only to compensate for these "cold losses" in existing cooling devices 0·2–0·3 kWh must be spent, i.e., about three to four times more than for the isolation itself. This shows that an apparatus which has to compensate for the "lost cold" is extremely imperfect. When this was established, we found what one should start from i.e., in the first place cooling devices should be improved.

Before reporting how this problem was solved, I have to dwell upon a general description of contemporary plants for the production of liquid air.

In order to obtain air in the form of a liquid, it must, as already mentioned above, be cooled down to the temperature of −194°C. In cooling machines compressed air is being cooled by making it do external work as it expands. In this case the cooling of air was proved to be equivalent to the work it performed.

It turned out that it is rather difficult to construct an efficient machine which would operate at low temperatures. One has struggled with this problem many years. An ordinary cooling machine (gas-expansion machine) resembles very much a steam-engine, but, instead of steam compressed air is supplied to it when expanding, it performs work (pushes the piston) and gets cooled. However, in these machines it does not expand to normal pressure but only to 8–10 atm, and gets cooled not down to a temperature of −194°C but only down to −150°C.

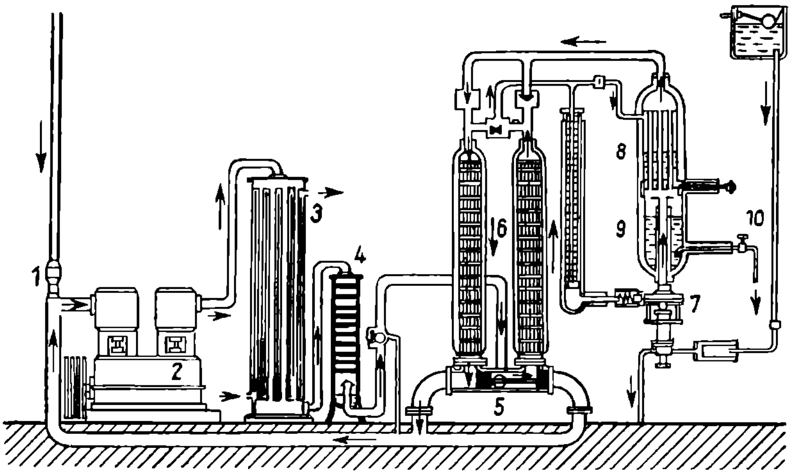

Fig. 1. General scheme of the arrangement for producing liquid air by means of the new turbine. Air passes through the filter 1 to the compressor 2, where it is compressed to 6–7 atm. The compressed-air stream passes through the water pipe cooler 3 and oil separator 4 and gets into the clack box of the regenerators 5. Leaving the valve the compressed-air stream gets into one of the regenerators 6, representing a tube filled with a large amount of metal gauze, called nozzle. From there the compressed air passes to the turbine 7, where it expands and gets cooled. Thereupon it comes back through another regenerator, to the nozzle of which it transfers its cold. Every half a minute the direction of motion of the air is changed by means of the switch valve 5. The air coming from outside enters the regenerators through which a cold stream has just passed. Thus, the air coming to the turbine is already precooled. As a result of successive repetitions of this process the air is cooled down to a temperature sufficiently low for the compressed air, going to the condenser 8, to be converted into liquid. The liquid air from the condenser gets into the container 9, from where it can be drawn off through the tap 10.

In order to cool air by a further 44°, i.e., down to its liquefaction point, one has to have recourse now not to external mechanical work of the gas but make use of the so-called Joule–Thomson effect, i.e., of the internal work done as the gas expands freely. Air, compressed to a high pressure (up to 200 atm), is pre-cooled down to −150°C by means of a cooling machine. Thereupon the air is allowed to expand freely to normal pressure. It then gets cooled on account of the internal work so much that a part of it goes over into the liquid state.

The main source of losses in contemporary plants is the low efficiency of the expansion engine. The cause of such a low efficiency lies in the fact that these engines are difficult to construct. For example, it is difficult to make the piston, tightly fitted on the cylinder, move with small friction, because at such low temperatures no lubrication can be applied.

Heylandt overcame this difficulty in the most witty way. In his machine the cylinder and piston are at room temperature. The cooled air during its short stay in the cylinder cannot lose much of its cold from its contact with the walls of the cylinder. But large cold losses can be prevented only when the pressure of the gas in the cylinder is sufficiently high, i.e., when its heat capacity per unit volume, which is proportional to the pressure, is large. It is evident that in this case the "amount of cold" in the gas will be large, while the surface of the contact of the gas with the warm walls of the cylinder will be negligible. That is why the heat penetrating through the walls of the cylinder will not increase substantially the temperature of the cooled gas. This accounts for the fact that in piston refrigerators the expansion goes from 200 atm to only 8–10 atm. The work that would be done by further expansion from this pressure to the normal one is lost. Consequently, the cold corresponding to the lowest temperatures is also lost.

Thus, in plants for fractionating air, operating with piston expansion machines, one has to apply air compressed to a high pressure. For the assembly compensating for cold losses one has to apply an additional high-pressure cycle. Although only about one tenth of the total amount of air passes through this assembly, it is rather bulky. This is accounted for by the fact that in such assemblies there operate high-pressure piston compressors, which are considerably more bulky than turbo-compressors operating on the centrifugal principle and applied at low pressures.

Thus, we come to the problem of finding out some other method of liquefying air, in order to manage without these high pressures. But is it possible, using only low pressure, to carry out the process of liquefaction of air with such a high efficiency that losses in cooling may be compensated for cheaply?

First of all the question as to which are the conditions under which such a process is possible at all is to be answered. It is evident that if we want to work at pressures of, say, 5–6 atm, we have to make it possible to obtain the entire cold, down to $-194°C$, on account of a mechanical work of the gas, because internal work can be used for cooling only in the case when the gas is pre-compressed to a high temperature. The only machine by which such a low temperature can theoretically be achieved is a turbine.

Turbines have a number of advantages over piston machines. The difficulty with lubrication no longer arises as bearings on which the rotor is rotating can be taken out by means of a long thin axis and kept at room temperature, whereas the turbine will rotate in the cold air.

This idea is in itself not new—the English physicist Rayleigh was the first to put it forward in 1898. But 40 years have elapsed since that time, and turbines are still not widely used for producing low temperatures. Attempts at applying them were made by the engineering works Linde in Germany and Claude in France, and turbine expansion machines were designed also in our country. But it turned out that the efficiency of all types of turbines with which the experiments were carried out was very low (60 per cent). With such a low efficiency at a low pressure one cannot expect to obtain a low temperature sufficient for cheap air liquefaction.

One had to consider the following question: why do turbines have such a low efficiency? Is that an inevitable result of the fact that they operate at low temperatures, or is that simply a shortcoming of the construction, a result of an improper design of the machine?

Till now this question has not been raised clearly, and for the production of low temperatures the most widely used type of an impulse steam turbine has been applied. An impulse turbine is none other than a nozzle from which gas is going out, and a number of blades pushed by the gas. It is the kinetic energy of the gas pushing the blades that is transformed into the energy of rotation.

How to obtain the highest efficiency of a turbine? We approached this problem theoretically, at first not going into the details of the design. We tried to find out theoretically on which physical factors do losses in the turbine depend.

Usually turbines were designed so as to be applied with steam. Certain losses dependent on the density of the gas were, in the conditions of a steam turbine, so small that they were not taken into account. But, as shown by our theoretical investigations, it is these losses that in refrigerating turbines become particularly important, because air at low temperatures becomes so thick that it resembles in some physical properties a liquid rather than steam. Losses due to irregular motion of the gas under the influence of centrifugal forces are especially enlarged.

Finding this out, we put ourselves the problem: can some benefit be derived from these impairing centrifugal forces, as in the case of a hydraulic turbine?

Thus we arrived at the idea that gas at low temperatures should be treated not as steam but rather as water, and that turbine expansion machines should be designed not like a steam turbine but like a hydraulic one. In this case, of course, it should not be forgotten that the gas nevertheless remains a gas with a number of properties characteristic of a compressed medium. Therefore we made up our mind to design a turbine whose capacity would consist of two parts, (1) action of the jet, and (2) use of the centrifugal force. Only this would double the capacity of the turbine with the same size.

These theoretical considerations were to be checked in practice. At our institute a turbine was constructed, combining the principles of both hydraulie and impulse turbines. This turbine was very small. Its rotor has a diameter of only 8 cm, and a weight of about 250 g. The entire turbine weighs a few kilograms, but about 600 m^3 of air per hour pass through it. The turbine does about 40,000 rev/min. Such a small size of the turbine is accounted for just by the fact that air at low temperatures has a higher density.

Our experimental turbine did start working right away. As is always the

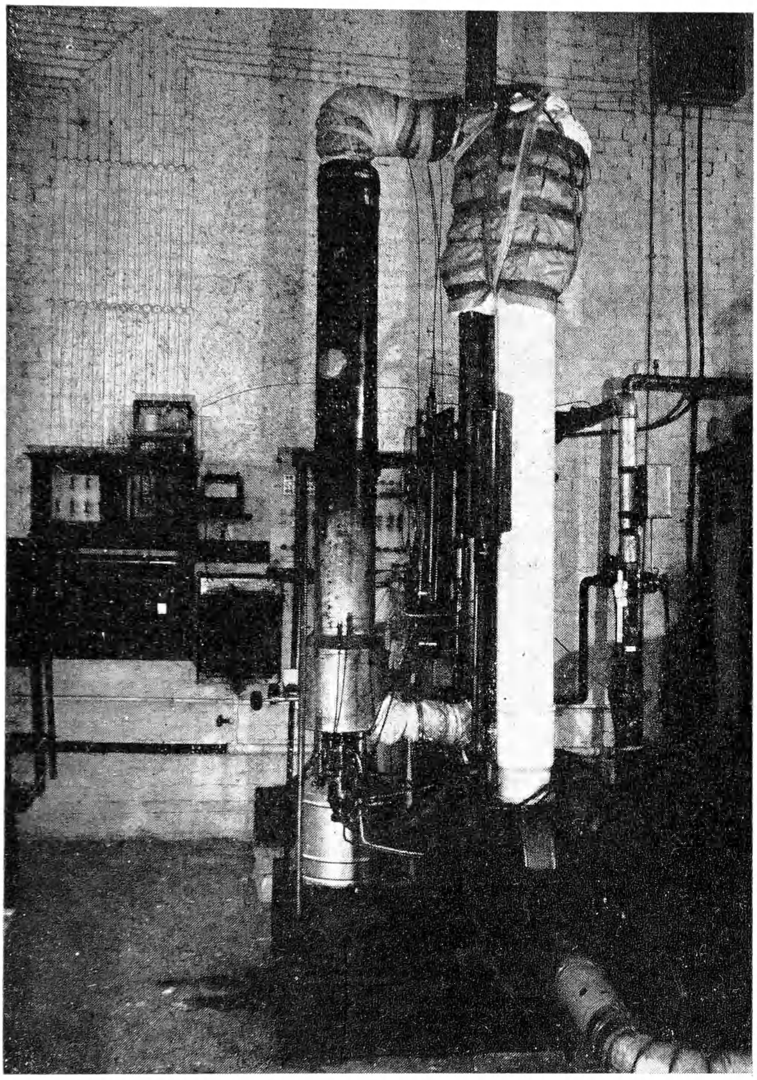

PLATE 11. General aspect of the new apparatus for liquefying air.

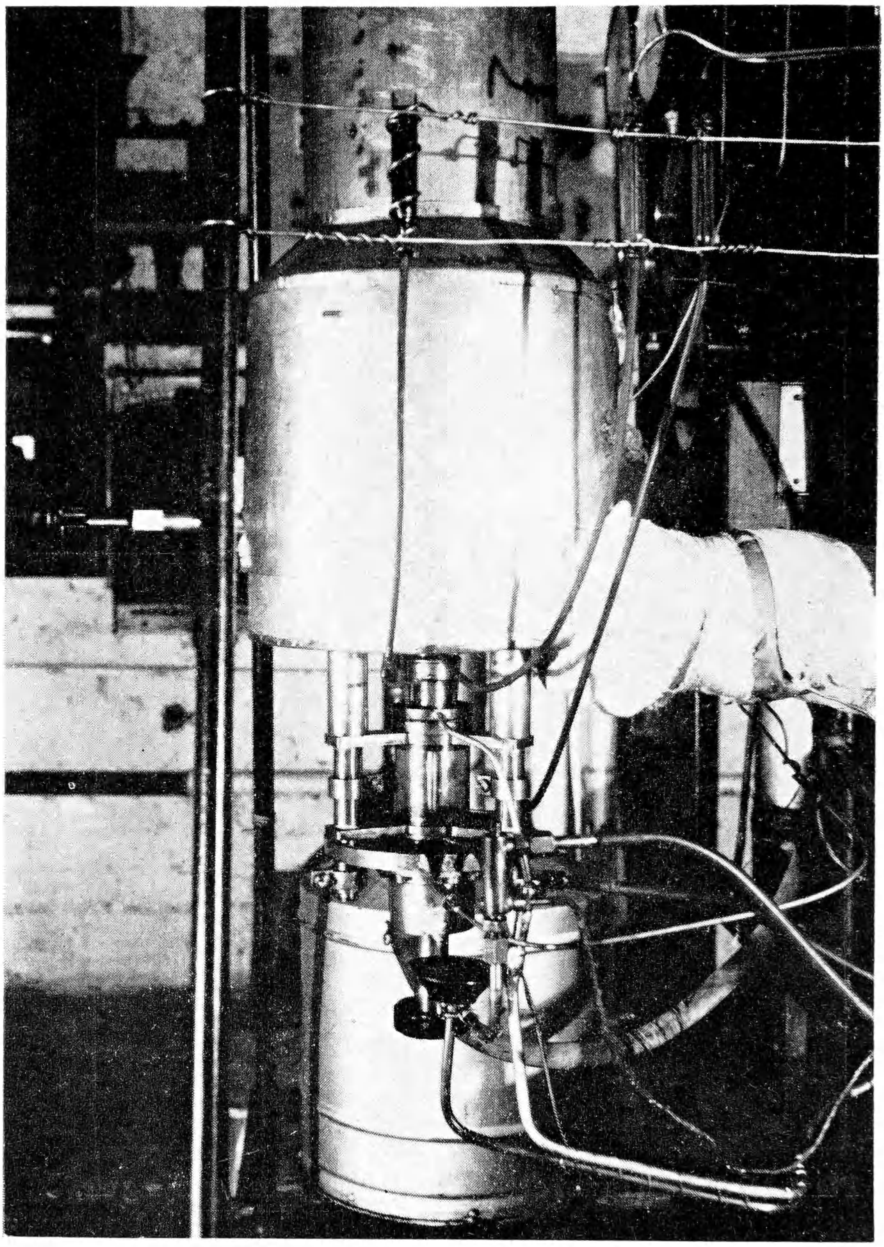

Plate 12. The new turbine for producing liquid air distinguishes itself by its compactness. The diameter of its rotor is only 8 cm. The entire turbine weighs a few kilograms.

case in research, difficulties came not from where they were expected. The first serious difficulty consisted in the fact that a stable motion of the turbine could not be achieved and vibrations could not be suppressed. And this is very important, because at a large number of revolutions per minute the velocity of motion of the external end of the rotor reaches 200 m/sec, which approaches the flight velocity of shot. However, in order to avoid "low temperature losses" the gap between the rotor and casing must be made very small (0·15 mm). For such a small gap and large velocities any vibration of the turbine is very dangerous.

In solving this problem we encountered a number of phenomena an explanation of which not only allowed us to make the motion of the rotor of our turbine perfectly stable but, maybe, will also contribute to establish a more stable motion in large rapidly revolving turbines. This may entail a decrease of the gaps and admittances and, consequently, also an increase of the efficiency of turbines.

Tests of the turbine constructed at our institute showed our theoretical assumptions to be completely valid: the efficiency of our turbine proved to be higher than 80 per cent, in spite of its small size. It should be noted that in large turbines the efficiency is always higher than in small ones, because in the former the ratio of the surface to the volume is lower. Consequently, since the amount of treated gas is proportional to the volume, whereas losses are proportional to the surface, in such turbines losses are also lower.

The new device operating at low pressures possesses many advantages; it distinguishes itself by its safety and a high reliability, makes it possible to apply turbo-compressors and to manage without a preceding purification of air from admixtures of carbonic acid and moisture. It is known that regenerators at low pressures need no purification of air from these admixtures. The latter are deposited in regenerators when air comes in, but are blown back by the outgoing current of air, which simplifies considerably the arrangement.

The new apparatus makes it possible to produce liquid air in a more economical way. It is eight to nine times lighter and considerably cheaper than an ordinary device.

Our experimental arrangement has been in operation already for 9 months, and has been supplying our institute regularly with liquid air. It begins to liquefy air in 18–20 min after the start, whereas usually the process of liquefaction begins after several hours.

Thus, the new turbine yields a more economical way of producing liquid air. This in itself solves one of the basic problems associated with the production of oxygen from air. At present the institute is already working on other problems, the solution of which may enable us to obtain oxygen at a lower cost. We take as a principle of this work the advantages presented by the turbine that we constructed.

8. A NEW METHOD OF PRODUCING LOW TEMPERATURES FOR AIR LIQUEFACTION

In spite of the fact that air was first liquefied by Cailletet and Pictet as far back as in 1877, it began to acquire technical importance much later. The Pictet cascade method, based on the evaporation principle, is quite efficient, but, because of its complexity and cumbersomeness it could not be widely used, the more so as liquid air had no industrial applications at that time. It is only since 1895, when Hampson simultaneously with Linde invented a simple type of liquefier operating on the principle of cooling by means of the Joule–Thomson effect, that liquid air began to be applied ever more widely, mainly as a cooling agent in laboratory practice. Nowadays it is indispensable in most scientific research.

If the application of liquid air were confined to its use as a cooling agent, then demand for it would hardly be great, and the main factor in designing liquefiers would be their simplicity rather than the economy of the processes of liquefaction. It is just Linde's and Hampson's machines that satisfied the requirement of simplicity, and it is this that accounts for the fact that they are often used, even nowadays, for producing liquid air in small amounts, in spite of the low efficiency of the cycles underlying their operation.

In 1899 a new and extremely important industrial application of liquid air was found. It is based on the following phenomenon, discovered almost simultaneously by Linde and Baly. Owing to the fact that the boiling point of nitrogen at atmospheric pressure is lower by 12·8° than that of oxygen, liquid air during its evaporation gets enriched with oxygen. Linde took this phenomenon as the principle of his technological method of fractionating oxygen and nitrogen from air.

With the progress in scientific elaboration of the methods of fractionating air and with the discovery of a possibility of producing oxygen at a low cost, demand for it from industry, in particular for autogenous work, began to increase rapidly. The demand from industry for oxygen is readily accounted for, because it is connected with the process of combustion, which is the basic process in a number of branches of the national economy. Air contains besides oxygen, which is directly necessary for the combustion of fuels, also four times more inert nitrogen, which not only harmfully lowers the temperature of

П. Л. Капица, Новый метод получения низких температур для ожижения воздуха, *Советская Наука*, 5, 150 (1939).

combustion but also gives rise to heat losses (leaving the furnace, nitrogen carries away heat). Calculations show that in a number of cases even a partial enrichment of air with oxygen may considerably increase the intensity and economy of thermo-technological processes. Hence there appears contemporary demand for oxygen and oxygen-enriched air on the part of metallurgy, energy economy and gasification of coal and oil, reaching ten thousand or so cubic metres of oxygen per hour. There is just as much demand for nitrogen separated from oxygen, which is the basic product of the chemical industry of bound nitrogen, indispensable for agriculture in the form of nitrogenous fertilizers.

Further, in producing nitrogen and oxygen from air, also rare gases such as argon, neon, krypton and xenon are obtained as by-products of this process. From the industrial point of view, some of these gases are particularly important, being the best fillers of electro-vacuum devices and filament lamps.

Such a high demand from industry for oxygen puts before science the problem of the most economical methods of producing these gases from the standpoint of cheapness (capital investment) and efficiency (energy losses) of the apparatus.

Before proceeding to a more detailed consideration of the Linde method of fractionating air, the following question is to be answered. Is free air the best source of raw material for obtaining oxygen and nitrogen?

This question can be answered with a fair certainty affirmatively. In producing nitrogen and oxygen, energy expenditure is the major cost. Therefore, nitrogen and oxygen must be chosen in a natural state such that the work necessary for their isolation will be the smallest. For oxygen and nitrogen in chemical combinations or for their solutions, for example, in water, the degree of binding is by far higher than in air. To isolate nitrogen and oxygen from air one also has to spend a certain work, which cannot be smaller than a quite definite minimum, but, because it is equal to only the work of compression, necessary to bring the partial pressure corresponding to its concentration to the normal one, it is relatively small. Calculations show that this work is smaller than for all other known sources from which oxygen or nitrogen could be obtained.

Another question of not less importance is the following. Is the process of fractional distillation of liquid air the most convenient one for obtaining nitrogen and oxygen from the atmosphere?

Contemporary physical concepts of processes taking place in distillation lead us apparently to the conclusion that the fractionation of nitrogen from oxygen is most efficiently carried out in the liquid phase. As a matter of fact, when we have to fractionate a mixture of two gases, e.g. oxygen and nitrogen, we have to introduce the factor counteracting the force of diffusion of these gases conducive to their intermixing. This force is the larger, the higher the temperature. Theoretically it appears to be possible, making use of the difference between the molecular weights, to fractionate nitrogen from oxygen, for example, by centrifuging the gas, or to change the nitrogen to oxygen concentration ratio, taking advantage of the fact that oxygen molecules are paramagnetic whereas nitrogen molecules are diamagnetic, by applying a magnetic field to the air.

Such assumptions have frequently been made. Theoretically, these methods are possible, but, because centrifugal forces produced in centrifuges as perfect and stable as possible, are insufficient to change the relative concentration of nitrogen and oxygen in the air, they are of no practical interest. Also, the action of the strongest magnetic fields is negligible in comparison with the forces of diffusion and, hence, is inapplicable for the fractionation of air.

Thus, the fractionation of nitrogen and oxygen is apparently possible in practice only by making use of molecular forces of affinity. When air is cooled down to liquefaction, most oxygen molecules first go over into the liquid phase, because their mutual attraction is stronger than that between nitrogen molecules or between nitrogen and oxygen molecules. Therefore the liquid fraction has more oxygen than the gaseous one. It is in several repetitions of this process that the fractionation of air consists. This process can be carried out continuously and theoretically completely reversibly. Consequently, the surplus of energy expenditure for this process over the theoretical minimum is due completely to imperfections of the apparatus, which give rise to losses. The main source of these losses lies, of course, in the requirement that the fractionation should be carried out at a very low temperature.

The fractionation of air, making use of molecular forces of affinity, but at a temperature differing less from room temperature, is feasible only in the presence of a special absorber. If we had a substance with a surface such that the affinity to it of nitrogen and oxygen molecules would be different, while the forces of affinity would depend on the temperature in a temperature interval closer to the room temperature than the liquefaction point, then the fractionation of air would be possible also at more convenient temperatures. The process would proceed in about the same way as, for example, the fractionation of air from helium by means of active charcoal. It is known that when a mixture of air and helium passes through charcoal at a low temperature, the air gets absorbed by the charcoal whereas the helium goes out. In principle, one cannot exclude the possibility of the existence of a substance possessing such a specific property of its surface with respect to nitrogen and oxygen, but, even if it were discovered, one could hardly expect a successful practical use of this method for large technological arrangements. Two basic difficulties can be foreseen. The first difficulty would lie in carrying out a continuous closed cycle. The second would consist in large losses associated with the heating of the absorbing substance itself, the amount of which would exceed that of the gas to be treated. From these considerations the fractionation of air by fractionating the liquid phase is to be assumed as the most economical one at present, unless new, still unknown properties of matter are discovered.

Therefore scientific and technological thought, as it appears to me, can most efficiently contribute to an improvement of this method, simplifying it and reducing losses, so that the energy spent for the fractionation of air may be closer to the theoretical minimum. The work described below at the Institute of Physical Problems of the Academy of Sciences of the U.S.S.R. on a turbine expansion machine is one of a number of studies aiming at an improvement on the method of fractionating air by means of the fractionation of the liquid phase.

The minimum work necessary for fractionating air into oxygen and nitrogen is calculated from the basic equations of thermodynamics, by making use of an expression which is usually associated with the name of Gibbs. This work is numerically equal to that spent for the isothermic compression of oxygen and nitrogen from their partial pressures to normal pressure. Assuming approximately that the partial pressure of oxygen in air is about 0·20 atm, while that of nitrogen is 0·80 atm, we have that for production of 1 m^3 of oxygen and 4 m^3 of nitrogen from 5 m^3 of air at least 0·068 kWh must be spent.

In the best of existing large Claude or Linde machines for the fractionation of air, 0·45–0·50 kWh would be spent for producing 1 m^3 of pure oxygen, i.e. 6–8 times more than the theoretical minimum. Such a low efficiency of these machines, amounting to 0·14–0·15, shows that scientific research has much to do in improving it.

The analysis of losses in such machines is well known, and we shall dwell upon it, because we shall need it for a further discussion of the development of our work. The process of fractionation of air is divided into three stages. First, the liquefaction of air; second, the process of fractionation consisting in repeated distillations and condensations, after which gaseous nitrogen and liquid oxygen are obtained; third, if one wants to obtain as a final product gaseous oxygen, then the liquid phase is to be evaporated, and the process must be carried out in such a way that the cold, accumulated in liquefying the first portion of air, should not be wasted with outgoing gaseous nitrogen and oxygen but be used for the liquefaction of incoming air.

Because of the imperfection of heat exchangers these processes proceed in such a way that there appear continuously cold losses, for the balance of which work should be spent. Therefore one of the basic constituents of a machine for the fractionation is the apparatus producing liquid air. In the starting period it produces the necessary amount of the liquid, and thereupon it compensates for its losses, maintaining the amount of the liquid in the distillation device constant. The role of this liquefier is very important, because up to about one half of all losses come from cold losses. Other losses, due to irreversibility, are also important, but we shall not consider them in the present work.

Cold losses consist of two parts, (1) incomplete heat exchange of inlet and outlet gases (these losses, due to under-recuperation, depend mainly on the type of heat exchangers but not on the capacity of the machine), and (2) losses due to heat conduction to the surrounding medium. These losses depend to a considerable degree on the size of the machine. In large machines, where the surface to volume ratio of the machine is lower than in smaller machines, these losses will also be lower, because the amount of the treated gas is proportional to the volume, whereas losses are proportional to the surface through which the heat transfer takes place.

Depending on the size of the machine, total cold losses should be assumed to vary from 2·5 to 1·0 kcal per 1 m^3 of the treated air. Consequently, in producing 1 m^3 of gaseous oxygen these losses will lie between 12·5 and 5·0 kcal. In order to obtain such an amount of cold in an adiabatic way at the temperature of liquid air 0·040–0·016 kWh must be spent. As we shall see later, in contemporary liquid-air machines the efficiency is about 0·15, hence, in fact, 0·27–0·11 kWh are to be spent to balance cold losses, i.e. the expenditure is almost 4 times larger than the work (0·068 kWh) which is theoretically necessary for the fractionation. By improving the recuperation and thermal insulation the losses can certainly be reduced, but this will require an increase in the size of the machine, hence it is important to raise the low efficiency of that part of the machine which serves for balancing liquid-air losses.

There are also other very serious grounds calling for an improvement of the methods of balancing cold losses, adopted in contemporary fractionation machines. Air, coming into a fractionation apparatus, is usually divided into two parts. The major part (up to 90 per cent) is compressed to 5–6 atm and passed into the distillation device. The remaining part is compressed to a high pressure—usually to 200 atm—and passed to the cooling circuit. The high pressure is necessary because it is only at such a pressure that liquefaction of air by contemporary methods can be carried out. The necessity of using two pressures, one of these being up to 40 times higher than the other, makes the apparatus very complex. For example, for a gas at a high pressure, a set of tube heat exchangers must be used, whereas for the heat exchange of a gas at a low pressure one applies regenerators, having a considerably higher efficiency than tube heat exchangers. In working with usual methods, not only two sets of heat exchangers are necessary but also two compressors, one for low pressures and the other for high pressures. In machines treating a large amount of gas, of the order of 10,000 m^3/hr, for basic streams it is convenient to apply turbocompressors, which have, as compared with piston compressors, a higher efficiency, a considerably smaller size and a more reliable operation. For a gas to be compressed to a high pressure the compressors must be of the piston type, but, for the production of several thousand cubic metres per hour, they have to be of an extremely large size. Hence it is evident that the elaboration of a method of producing liquid air with a high efficiency at low pressures will make it possible to apply only one compression and to change to turbocompressorse which will, undoubtedly, simplify and improve the operation of the apparatus, for the fractionation of air.

Putting before ourselves the task of elaborating a highly efficient cycle for the liquefaction of air at a low pressure, we have first to find out the sources of losses and their weight in determining the efficiency. The methods of designing liquefying machines, presently adopted in technology, are usually based on calculations of the thermal balance, and do not take into account the thermodynamical efficiency of individual processes taking place in a chosen cycle. We shall discuss the elements of a high-pressure cycle and a low-pressure cycle from the standpoint of irreversibility, and shall point out the sources of basic losses. This will point out a trend of research leading to an improvement of the cycle.

For our calculations we make use of the following expression, which is usually applied in technological thermodynamics. If we have a gas in a state determined by entropy S_x, heat content i_x, pressure p_x and temperature T_x, and pass it into a state determined by S_y, i_y, p_x and T_y, then, for a reversible process, the work which must be spent will be equal to

$$AL_{xy} = T_x(S_x - S_y) - (i_x - i_y), \tag{1}$$

where A is the mechanical equivalent of heat.

The work L_{xy} is, obviously, the minimum that is possible. But, in fact, the work spent in real conditions will be larger, and we shall denote it by $\mathscr{L}_{xy}$. The thermodynamical efficiency of the process will, evidently, be determined as the ratio of the ideal to real work. We denote it by η_{xy}, and obtain

$$\eta_{xy} = \frac{L_{xy}}{\mathscr{L}_{xy}}. \tag{2}$$

To calculate the minimum work necessary for compressing 1 kg of air at a pressure of 1 atm, we substitute into eqn. (1) the difference between the values of the entropy and heat content in the gaseous and liquid state. We denote this work by L_a. We obtain:

$$L_a = 0{\cdot}19 \text{ kWh/kg at } p_a = 1 \text{ atm};\ T_a = 293°\text{K } (20°\text{C}). \tag{3}$$

Under real conditions, for example, in a Heylandt apparatus, if losses due to the compressor are taken into account, $\mathscr{L}_a = 1{\cdot}3$ kWh and more is spent per 1 kg of liquid air, so that the thermodynamical efficiency of the apparatus, according to eqn. (2), is $\eta_a = 0{\cdot}145$.

In what follows we shall see that such a low efficiency of this cycle is accounted for mainly by the fact that use is made of cooling processes with a very low reversibility.

It is known that for producing cold in a reversible way the expanding air does external mechanical work. Machines in which the expanding air does mechanical work and owing to this gets cooled are called detanders. The only type of these machines, which has up to now been used for liquefying gases, are piston detander.* In essence, they represent a steam engine, operating not with steam but with a compressed gas. In the most widely used type of these engines—the Heylandt detander—air comes in at the room temperature and a pressure of 200 atm, does work in expanding, and gets cooled down to not below −150°C. Since the temperature of liquid air is −194°C, the cooling down by the remaining 44°C must be now carried out on the account of the internal energy of the compressed gas, using the Joule–Thomson effect. This last process is thermodynamically not very efficient at ordinary temperatures, but becomes more efficient if the air is pre-cooled and if the expansion is carried out from high pressures. That is why the necessity arises of using pressures up to 200 atm.

The role of high pressures in liquefying a gas can be demonstrated as follows. The mechanical work which must be spent for liquefying air at a pressure of 200 atm can be calculated from eqn. (1). For this we have to cool air from the temperature of 293°K at a pressure p_b = 200 atm down to a temperature such that its heat content will be equal to that of liquid air at normal pressure. Then from the T–S diagram we obtain, according to eqn. (1),

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* Detander = expansion machine.

$$L_b = 0{\cdot}092\ \mathrm{kWh/kg}; \quad p_b = 200\ \mathrm{atm}. \tag{4}$$

Comparing this work with L_a, i.e. with the work which must be spent for liquefying air at normal pressure, eqn. (3), we see that it is more than twice less. Thus, the advantage of a high pressure in liquefying air is obvious, since in this case the work of the cooling cycle is considerably reduced, and its efficiency is of a lower importance.

Consequently, in the process of liquefaction the work spent is divided into two parts. The first part is spent for compressing 1 kg or air at a temperature T_a from a pressure p_a to p_b, and we denote it by C_b. For the ideal cycle it is calculated from eqn. (1) by substituting the corresponding initial and final values of the entropy and heat content. The second part is spent for the cooling cycle, and goes to the liquefaction of air at a pressure p_b. For the ideal process we denote the second part of the work by L_b. If the isothermal efficiency of the compressor is denoted by η_x and the thermodynamical efficiency of the cooling apparatus by η_t, then the actual work spent for liquefying 1 kg of air will be

$$\mathscr{L}_b = \frac{C_b}{\eta_b} + \frac{L_b}{\eta_t}. \tag{5}$$

For 1 kg of air compressed to pressure $p_b = 200$ atm, we obtain from the T–S diagram

$$\mathscr{L}_b = \frac{0{\cdot}12}{\eta_b} + \frac{0{\cdot}092}{\eta_t}\ \mathrm{kWh/kg}. \tag{6}$$

Assuming the efficiency of the compressor to be $\eta_b = 0{\cdot}59$, we find that, with the actual expenditure of $\mathscr{L}_b = 1{\cdot}3$ kWh/kg for the liquefaction, 1·1 kWh are spent for the cooling cycle, instead of the minimum of 0·092 kWh from eqn. (4). Consequently, in Heylandt machines the efficiency of the cooling cycle is $\eta_b = 0{\cdot}084$, i.e. 1·7 times less than that of the overall liquefaction cycle.

The cause of such a low efficiency of the cooling cycle, as can be shown, lies mainly in the imperfect operation of piston detanders: all other losses are about four times less than those due directly to the detander cycle. Thus, the main source of losses in contemporary machines is undoubtedly the low efficiency of the detander itself.

The cause of such a low efficiency of piston detanders lies in the well-known technical difficulties in constructing these machines. The difficulties consist in lubricating the piston, moving tightly in the cylinder, since there are no lubricants for low temperatures. Heylandt overcame this difficulty by allowing the cylinder and piston to be at room temperature, while the cooled air during its short stay in the cylinder cannot waste cold in touching the walls. This turns out to be possible to carry out without large losses only when the heat capacity per unit volume of the gas in the cylinder of the detander is large. Hence, in order that these losses may not assume too large values, in the Heylandt detander the expansion of air is carried out only from 200 to 10–8 atm, and the work which the gas could do in expanding from this pressure to the normal one is lost. Consequently, the corresponding cold is lost, which, as we have seen, is the most precious cold, since it is obtained at lower temperatures.

We proceed now to the consideration of the cycle of air liquefaction at low pressures. Such a cycle has not been carried out before our work for the following reasons.

First, the already low efficiency of piston detanders, which, as we have shown, is obtained at higher pressures, decreases even more at low pressures.

Second, in liquefying air at a low pressure the work spent in the cooling cycle is larger, since the gas is being liquefied at lower pressures.

The third reason lies in the requirement that the overall process should go on account of the cold from the detander, since cooling on account of the Joule–Thomson effect cannot take place because of its small value at a low pressure. Consequently, the detander has to produce cold at a temperature lower than the liquefaction point of air.

Thus, the requirements required from the detander in low-pressure liquefying plants are a high thermodynamical efficiency and operation at a low temperature. The only possibility of realizing such detanders, as it seems to us, is the use of the conversion of the work of compressed gas into cold in turbo-mechanisms.*

The turbo-machine for producing cold is called turbo-detander. In what follows we shall describe the methods that we adopted in constructing such a turbo-detander. Owing to the high efficiency of the latter and the fact that it can produce cold at a very low temperature, a cycle for liquefaction of air without the use of the Joule–Thomson effect could be carried out. From experimental data from an investigation of this turbo-detander and from an analysis of the thermodynamical characteristics of the cycle we obtain the following relative characteristics of machines operating at high and low pressures:

1. For one and the same efficiency of an ordinary piston compressor, $\eta_c = 0{\cdot}59$, the efficiency of the cooling cycle of a turbo-detander, according to our experimental data, will be 0·41 as against 0·148 for a piston-detander. This difference is quite sufficient to compensate for the increase in the work necessary for the cooling cycle, which is associated with the liquefaction of air at low pressures.

2. Low pressure allows one to make use of turbo-compressors, which are cheaper and simpler than piston compressors, especially when the latter operate at high pressures.

3. Heat exchange between the inlet and outlet gas in low-pressure cycles proceeds without additional losses, since both streams have a constant and the same heat capacity. In working with high pressures, changes in the heat capacity of air, which at 200 atm and room temperature is $c_p = 0{\cdot}33$ whereas

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* A complete account of the mathematical basis of the design is published in the *Zh. Tekhn. Fiziki* (*J. Techn. Phys.*), 9, No. 2, 1939 (see *Collected Papers of P. L. Kapitza*, vol. II, No. 36, p. 521).

at a low temperature it amounts up to $c_p = 0{\cdot}44$, make it impossible for the heat exchange to proceed without losses. Calculations show that the value of these heat-exchange losses in high-pressure cycles covers a considerable part of losses in low-pressure cycles, which are due to the necessity of producing cold at a low temperature.

4. Losses due to the under-recuperation at low pressures will, in general, be more appreciable than at a high pressure, since the efficiency of production of cold per 1 kg of air is about three times lower. This under-recuperation is easily compensated by the fact that in working with the gas at a low pressure use can be made of regenerators as heat exchangers, which is impossible for technical reasons at high pressures. In addition to a number of advantages in running (as, for example, the fact that the gas does not previously have to be purified from carbonic acid and dried), regenerators are far more efficient heat exchangers. Under-recuperation is for them 3–4 times lower than for ordinary heat exchangers and, hence, heat-exchange losses in working at a low pressure are lower rather than higher.

5. At low pressures it is losses due to the pressure drop in the pipeline that are most pronounced, but again the use of regenerators allows them to be reduced to a very small value, because the hydrodynamical resistance of a regenerator is very small in comparison with heat exchangers. Furthermore, external cold losses due to heat conduction can affect appreciably the efficiency of the machine only when the latter is small. Therefore a liquefying turbo-plant will be most efficient when treating large amounts of gas; moreover, in this case use can be made of compressors of a high efficiency.

Thus, having constructed a turbo-detander of a good thermodynamical efficiency, we open the possibility of producing a low-pressure liquefaction cycle of a higher efficiency than a high-pressure cycle.

The idea of using a cooling turbine as a detander dates back to the time of the earliest development of cooling techniques. Rayleigh was the first to point it out in 1898. However, up to now turbines have found no application for the liquefaction of air. Advantages of a turbine are obvious. This is a mechanism which needs no solution to the problem of lubrication at a low temperature, since the bearings in which the axis of the rotor is rotating can be drawn out of the cool region of the rotor to a distance sufficient for them to work under normal conditions. The main difficulty in applying turbines for the processes of liquefaction of air consists in the fact that all turbines constructed up to now were of a low efficiency and hence unsuitable for the processes of liquefaction. In the most perfect plants for fractionation of air use is made of turbo-detanders as additional sources of cold, to balance heat losses. However, up to now one has not succeeded in producing the necessary cold by turbines.

The technical efficiency of detanders used in these plants is about 0·6, even for a considerable capacity. Such a low value of efficiency, lower than that encountered in other turbines operating with steam or water, raises the question as to whether this is an inevitable consequence of a turbo-detander's operation with air at low temperatures or a result of an inappropriate design of these machines.

All detanders so far described in the literature were designed like impulse steam turbines with an axial motion of air. Upon considering the problem of a proper choice of the type of turbine for the cooling cycle, we shall see that a choice of design after an impulse steam turbine is inappropriate and that, from theoretical considerations, a turbine operating at low temperatures with a proper choice of design must have an efficiency not only higher than that of presently available turbo-detanders but exceeding even that of steam turbines.

The point is that all basic losses which must be taken into account in steam turbines are lower in a turbo-detander, as shown by calculations.

But there are also losses which reach a large value just in turbo-detanders, if the design of the latter is not properly chosen. These losses are first and foremost due to the friction of the rotor against air. They will be 16 to 100 times larger in turbo-detanders than in steam turbines. In order to reduce them, first of all the radius of the rotor for turbo-detanders should be chosen as small as possible.

In turbo-detanders there is one more form of losses, which in steam turbines are usually not taken into consideration because of their smallness. The point is that the density of air in turbo-detanders at a working temperature of 100°K is eleven times higher than that of steam at the same pressure and a temperature of 600°K. When performing circular motion such a dense medium will be acted upon by a considerable centrifugal force, which will produce a radial pressure gradient; air will be thrown out of the rotor, and this will interfere with a regular motion of the gas in the blades and will give rise to losses.

Calculation shows that this is the basic source of losses, which has so far, maybe, not been taken into consideration and, hence, turbo-detanders designed on the principle of axial turbines give such a low efficiency.

A logical consequence of taking into consideration the harmful effect of centrifugal forces should be an effort to derive some benefit from this phenomenon.

An example of turbines in which the working medium is of a high density and is flowing slowly are water turbines. In them the action of centrifugal forces increases that of the water flow.

Thus, in view of the high density of air and the low velocity of its flow in turbo-detanders, it should be treated rather as water, but bearing in mind that it is nevertheless a gas, i.e. a compressible medium. Also this property of the air can be used advantageously, for example, for turbo-detanders with a large pressure drop, applying the principle of Laval nozzles. Thus a properly chosen type of turbo-detander will be as if a compromise between a water turbine and a steam turbine.

It can be shown that in a turbo-detander the action of the gas is equivalent to that of centrifugal forces, and, making use of the effect, the capacity of the turbo-detander can be increased two times for the same size of the rotor and nozzles. In this case losses will remain unchanged and, consequently, relative losses will decrease by a factor of two. Thus, making use of centrifugal forces, one can design a turbo-detander of a very high efficiency.

Comparing a turbo-detander with a water turbine it can be shown that they resemble each other much more than the former to a steam turbine. The kinematic viscosity of air at a low temperature is close to that of water, hence relative losses in turbo-detanders must be of the same order as those in water turbines, where, as is known, they are rather small, while the efficiency, which is high even for turbines of a medium size, reaches up to 0·87.

An experimental check of the validity of the above theoretical considerations was carried out on a turbo-detander designed and built at the Institute of Physical Problems of the Academy of Sciences of the U.S.S.R.

Calculations on turbo-detanders show that, owing to the high density of air and, consequently, a large value of the kinetic energy per unit volume of the inlet stream, the machine is to be of a very small size. Thus, for example, the turbo-detander that we constructed, with a capacity of 500–1000 m^3 of air per hour at the normal pressure and temperature, has the rotor of a diameter of 8 cm, weighing only 250 g. The piston compressor, necessary for supplying air, weighs 3 tons and needs a power of 50–80 kWh. The necessity of having the rotor of such a small size is dictated primarily by that of reducing losses due to friction during rotation.

In constructing such a small detander a number of technical difficulties are encountered. Thus, in order to obtain a sufficient peripheral velocity the number of revolutions should be chosen to exceed 40,000 per min. Furthermore, in manufacturing such a small rotor it is difficult to make the blades of a complex but more suitable form, and for simplicity one has to make them rectangular.

Hydrodynamical rating of the blades and distributor is carried out, as usual, in turbines and air-blast engines, because our turbo-detander represents, so to speak, a reversed turbo-compressor. In this case one has to be guided by the physical properties of air at a low temperature (viscosity, density, and so on) and to choose such a form of the blades and rotor in which account is taken of the above theoretical instructions. The mechanical stability of the rotor is determined from the known conditions for rotors. Here it should be noted only that it is important to chose a material such that it should be strong and may preserve its plasticity at a low temperature. We chose monel (67 Ni; 29 Cu, other FeMn), but as was shown by the author,* austenitic steels are most suitable for the work at a low temperature (for example, EYa-1).

The general aspect of the turbo-detander construction is given in Fig. 1, in which the form of nozzles *1* and rotor's blades *2* is seen.

To insulate the bearings most efficiently from cold, as well as to reduce cold losses, the rotor is mounted on a flexible Laval shaft *3*, and a cantilever construction is chosen. Calculation of these shafts and critical velocities is well known in turbine engineering, and we shall not dwell upon it.

The small size of the turbo-detander requires a precise work and balancing of the rotor, which is associated with the necessity of having a small gap between the rotor and casing. At the outlet of the guide blade, air will have a pressure about two times higher than at the outlet of the turbine. In order to prevent air from flowing away from the blades, as shown in Fig. 1, the rotor has two labyrinth seals *4*, the gap in them being only 0·15 mm; the motion of the rotor must be sufficiently stable in order that no contact may occur here. The size of the labyrinth rings from both sides is taken the same, in order that no axial pressure may be exerted on the bearings. Insulation of cold air in the casing from the outside medium is achieved by means of the labyrinth seal on the axis *5*.

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* P. L. KAPITZA, *Proc. Roy. Soc.*, 1934; See *Collected Papers*, vol. I, No. 33, p. 455.

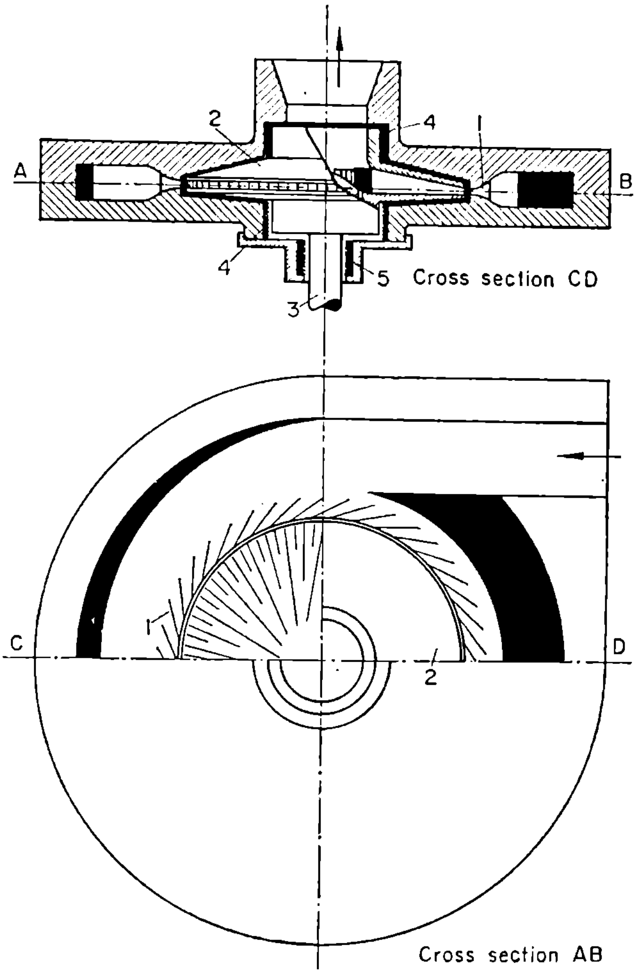

FIG. 1

The requirement of a high stability of the rotor in rotating with a large number of revolutions per minute gave rise to very serious difficulties, which for a long time interfered with the realization of the machine. Experience showed that when the number of revolutions exceeds considerably the critical one the stability is impaired, and the rotor strikes the casing. The cause of this phenomenon remained for a long time unknown, and only after a thorough investigation it was found that it results from the friction of the rotor surface against the surrounding air, which, owing to the high density of the surrounding air at a low temperature, reaches a large value. This problem is to be considered in more detail.

Rotating machines, such as turbines, turbo-compressors, centrifuges, gyroscopes, and so on, are ever more widely used in technology. Peripheral velocities of the rotors of these mechanisms often reach more than 200–300 m/sec, hence the problems as to their stability and vibration are very important.

Among these problems the greatest attention is usually paid to the phenomenon of passage through the critical velocity. This phenomenon is well known to turbo-engineers. It consists in the fact that, when approaching a certain, for a given machine quite definite number of revolutions per minute, the rotor begins to vibrate strongly, and if it is not well balanced the machine may even break up. After the passage through the critical velocity the machine again operates normally.

This process corresponds in its character to the phenomenon of resonance, when under the action of a periodic force arising from an eccentric position of the centre of gravity a disc performs forced oscillations. When the frequency ω of this force approaches that of natural oscillations of the system p, then, as in resonance, the amplitude of forced oscillations increases rapidly.

In practice, rotors usually have a complex configuration. In the presence of a number of discs mounted on one shaft, and taking account of the mass of the shaft itself, the system will have a series of normal frequencies, and to each of these there will correspond a critical velocity of rotation of the rotor and the accompanying phenomenon of instability. A further complication of the phenomenon is caused by the fact that when the shaft sags the discs will stoop, and this will give rise to gyroscopic forces which, as in the case of a gyroscopic pendulum, will change the frequencies of the normal oscillations of the system and, correspondingly, the value of the critical velocity.

For a stable operation of the machine it is very essential that the working velocity should not coincide with one of the critical velocities. Hence in designing the rotor much attention is paid to the calculation of the critical velocities, and these calculations become rather complicated in the case of multidisc turbines. Therefore in the technical literature much place is given to the problem of calculating critical velocities.

However, rather little attention has as yet been paid to problems associated with the process of passage through the critical point (for example, on the degree of accuracy in balancing rotors, on dangerous saggings of the shafts, and so on).

In constructing rapidly rotating turbo-detanders the author had in practice to cope with this problem and a number of other problems associated with the stability of the rotors. In working on these problems a number of conditions were found ensuring a smooth passage through the critical point as well as the stability of motion of the rotor for any number of revolutions per minute.

The most important and widely used process, giving the passage through the critical velocity even at arbitrary slow running of the machine, is due to the presence of damping forces, which confine the amplitude of forced oscillations to a finite value at the moment of passage through the critical velocity.

Stodola* was apparently the first to point out correctly the role of damping forces in the passage through the critical velocity, but his conclusion that this damping may take place owing to the friction of rotor's surface against air or steam is incorrect. Calculations show that this friction is not only insufficient but, if its effect is calculated in a correct way, on the contrary, causes an instability in the rotation of the rotor. Other attempts to investigate the effect of the friction in the passage through the critical velocity led to extraordinary complex calculations from which it was not possible to derive the physical basis of the process and no practical results could be obtained for the design and calculation of the needed accuracy of balancing rotors, necessary for the passage through the critical velocity.

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* STODOLA A., *Dampf und Gasturbinen* (1924).

In working out a turbo-detander for producing low temperatures the author had to cope with all these problems of stability. Owing to the high density of the surrounding medium at these temperatures the problems of the friction against the surrounding medium became very acute. The rotor of the turbine lost its stability, while the operation conditions required that the gap between the rotor and casing should be kept very small. Experimenting with a number of rotors under different conditions, the most essential elements of instability could be determined empirically, and the corresponding theoretical basis could be found for a method allowing the motion of rotors to be considerably stabilized.

We succeeded in finding theoretically the causes of the instability of motion of rotors, and showed that, when the damper which damps the transverse oscillations of the axis is correctly designed, a complete stability of the rotor's motion can be achieved in the passage through the critical velocity, and the instability due to the friction of the rotor against the surrounding medium can be avoided.

The theory of and experiments on this phenomenon are expounded by the author in another paper,* where also the damper is described by means of which the necessary stability of the turbine in rotation can be achieved. This damper is put on the cantilever end of the axis of the turbine, and confers to it the necessary great stability. In spite of the fact that the rotor's peripheral velocity reaches 180 m/sec while the gap at the periphery of the rotor and in the labyrinth seals does not exceed 0·33 mm and 0·15 mm, respectively, the rotation proceeds without sideway touching, which points to the degree of this stability.

The possibility of obtaining a more stable rotation in the machines by applying damping seems to me to be of promise in technology. However, even in mechanical engineering, little attention has as yet been paid to the effect of damping forces. The use of possibilities opened by this effect will undoubtedly improve appreciably the operation of a number of mechanisms.

A confirmation of this can be seen if only from the fact, as shown by us, that every rotor rotating in a casing with a limited gap at any velocity in essence performs an unstable motion and, if in usual rapidly rotating mechanisms there were no fortuitous damping forces produced in squeezing the lubricant in the bearing, the sagging of the shaft would increase slowly according to the equation of the logarithmic spiral until the rotor strikes the casing. Also, in the absence of damping, the passage through the critical velocity would in principle be impossible. Consequently, turbines, air blast engines and other rapidly rotating machines can operate only owing to the existence of the damping force, which we call rightly a fortuitous force, since it is not calculated and is produced accidentally by ordinary oil bearings. If the oil bearing is replaced by a ball-bearing, damping forces decrease and, hence, in spite of the high economy of ball-bearings they should not be applied in turbine-plants.

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* Influence of friction forces on the stability of high-speed rotors *Zh. Tekhn. Fiziki*, 9, No. 2 (1939); See *Collected Papers*, vol. II, No. 37, p. 551.

Owing to the high density of the medium surrounding the rotor and the increased surface friction the resulting instability in the described turbo-detander reached such a degree that the oil-bearing damping was no longer sufficient to suppress it. However, the introduction of damping not only eliminated this instability but also made it possible to pass over the ball-bearings and to reduce considerably the gap between the rotor and casing. I think this example illustrates all the advantages of an ordered damping, i.e. damping the value of which is calculated in advance, introduced by a special mechanism.

The introduction of ordered damping will not only facilitate the passage through critical velocities but will also make the machine less sensitive to external perturbing forces, for instance, to the vibration of the support. This, for example, allowed us to give up a base for our turbo-detander.

The axis, bearing the rotor, rotates in two ball-bearings, which are lubricated with a lubricant from a dropper. In order to relieve the bearings from any superfluous load, the rotor sits freely on the axis (on the sphere), so that it can bend about its centre of gravity, and, thus, the major axis of inertia is set automatically along the axis of rotation. The freedom of this sitting is limited by friction, so that accidental perturbations and vibrations will be damped quickly. The moment of rotation from the rotor to the axis is transferred through a star coupling of special construction.

Under such precautions it turns out that, for a single arm suspension of the rotor, the transfer of sideway forces to bearings is small, and even after the latter have worked for 700 hr at 40,000 rev/min they are in such a state that they are quite suitable for further work.

The work obtained from the turbine is not exploited usefully, but is taken up by a water brake, consisting of a small centrifugal water pump with an opening whose size can be regulated. Changing by this the load on the turbine, one can regulate the number of revolutions per minute. The rotor of this water brake has a diameter of only 3·2 cm, and weighs only 18 g; for a maximum load of the turbine it could consume up to 8 kWh. The pressure produced by the pump reaches 30–40 atm, and from its value by means of a manometer the number of revolutions per minute of the rotor is measured. In order to remove heat from the loading device, one passes through it water which, mixing itself with the water circulating in the pump, takes away the generated heat. Such an arrangement is very convenient for an experimental plant, because, measuring the flow rate of the flowing water and the difference between the temperatures before and after the loading device, one can determine the capacity. In the same way one determines the amount of cold produced by the turbine and consumed for liquefaction, since it obviously must be equal to the produced heat. By means of such an arrangement one can also establish quickly the optimum operation mode of the turbine, choosing working conditions to be such that at a constant flow rate the water may get heated as much as possible.

Experimental running of this turbo-detander for 700 working hours showed it to be a reliable mechanism, very simple to handle. It works normally with 40–42,000 rev/min, but was tested up to 60,000 rev/min. For the time being the turbo-detander uses 570 kg of air per hour. In the starting period up to 8 kWh are consumed, but at the working temperature about 4 kWh are consumed. The pressure drop at the turbine usually varies by about a factor of four.

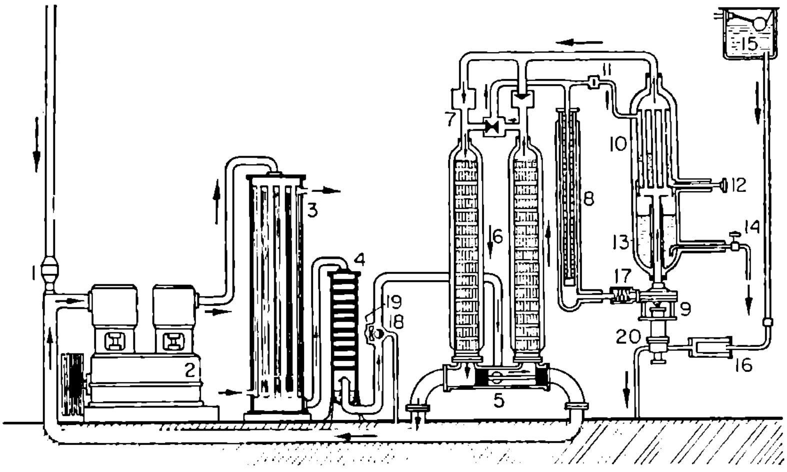

Fig. 2

The high efficiency of the detander (0·81) that we obtained confirms our theoretical premises. It should be noted that there is every reason to assume that, when turbo-detanders of a higher capacity are constructed, the efficiency will increase and may even exceed that of water turbines. For small turbines, like the one described, the efficiency is always lower, as is seen from theory and confirmed by practice in turbine engineering.

The cross section of nozzles and rotors increases with increasing size of the turbine and, hence, losses due to the pressure drop decrease with increasing size of the turbine. Relative losses due to the heat conduction also decrease with increasing turbine size, since they increase with the square of the linear dimensions, while the capacity of the turbine increases with the cube of linear dimensions. The relative gas leakage through labyrinth seals will also decrease with increasing turbine size. Finally, in constructing a large turbo-detander the technical possibility will be opened of making blades and nozzles of a form more efficient than those to be applied in small turbines.

But even with the presently obtained efficiency, one can count upon a successful use of the turbo-detander for the liquefaction of air. That this is so will be seen from the description of the experimental plant, given below.

Figure 2 shows the working scheme of this plant, constructed at the Institute of Physical Problems of the Academy of Sciences of the U.S.S.R.

From the dust filter *1* air passes to the compressor *2*, where it is compressed to 6–7 atm. This is a standard double-stage compressor BB-8 produced by the works "Borets", for a normal working pressure of 9 atm, with a capacity of 13·3 m^3 of air per minutc at 500 rev/min. It has not as yet been used by us at the full pressure and capacity, since it operates with a lower number of revolutions (380–390 rev/min), and its measured capacity is 9·5–10 m^3 of air per minute.

The stream of compressed air passes through the water pipe cooler *3* and oil separator *4* and gets into the clack box *5* of the regenerators *6*.

The valve distribution of the construction that we designed is of the type of a piston valve distributor with balanced pressure. It is put in operation by two brake-electromagnets KMP-14 produced by the works "Dinamo". The switching is made every 25–27 sec by means of a contact mechanism driven by a small motor.

The regenerators *6* have a filling somewhat different from that usually applied; instead of a crimped band we use a flat band with pimples, having a thickness of 0·1 mm and a width of 50 mm. The temperature drop at the warm end amounts to 2·0–2·5°C. The thermal insulation is of the vacuum type. The valve *7* at the cold end is automatical.

The liquefaction cycle corresponds to the one that we have already considered; its scheme was shown in Fig. 2. Leaving the valve *7* the stream of compressed air is divided into two parts. The basic stream goes through a filter and the angular temperature equalizer *8* (whose functions will be considered later in more detail) and gets into the turbo-detander *9*, where it expands, then comes back through the internal tubes of the condenser *10*, through the regenerator and goes out. The other part of the stream of compressed air passes through the back valve *11* to the condenser *10*, where it is liquefied.

The liquid air from the condenser flows through the tap *12* to the container *13*, from which it can be drawn off through the tap *14*.

The gas which during this process has evaporated joins before the condenser the basic stream going out of the turbine, so that its cold is used further for the liquefaction.

As seen from the scheme, the condenser is insulated from cold losses by vacuum insulation in common with the container for liquid air. The temperature equalizer *8* has the same vacuum insulation.

Water for cooling the brake *14* is supplied at a constant pressure head through the filter *16* from the tank *15*. This is made in order that the water flow rate should not depend on pressure variations in the network.

The actual appearance of the corresponding parts of the plant can be seen in Plate 13.

As is known from the theory and practice on operation of low-temperature regenerators, the air which takes part in the heat exchange needs no purification from moisture and carbonic acid, because the moisture and carbonic acid which are deposited when the air stream comes in are almost completely blown out by

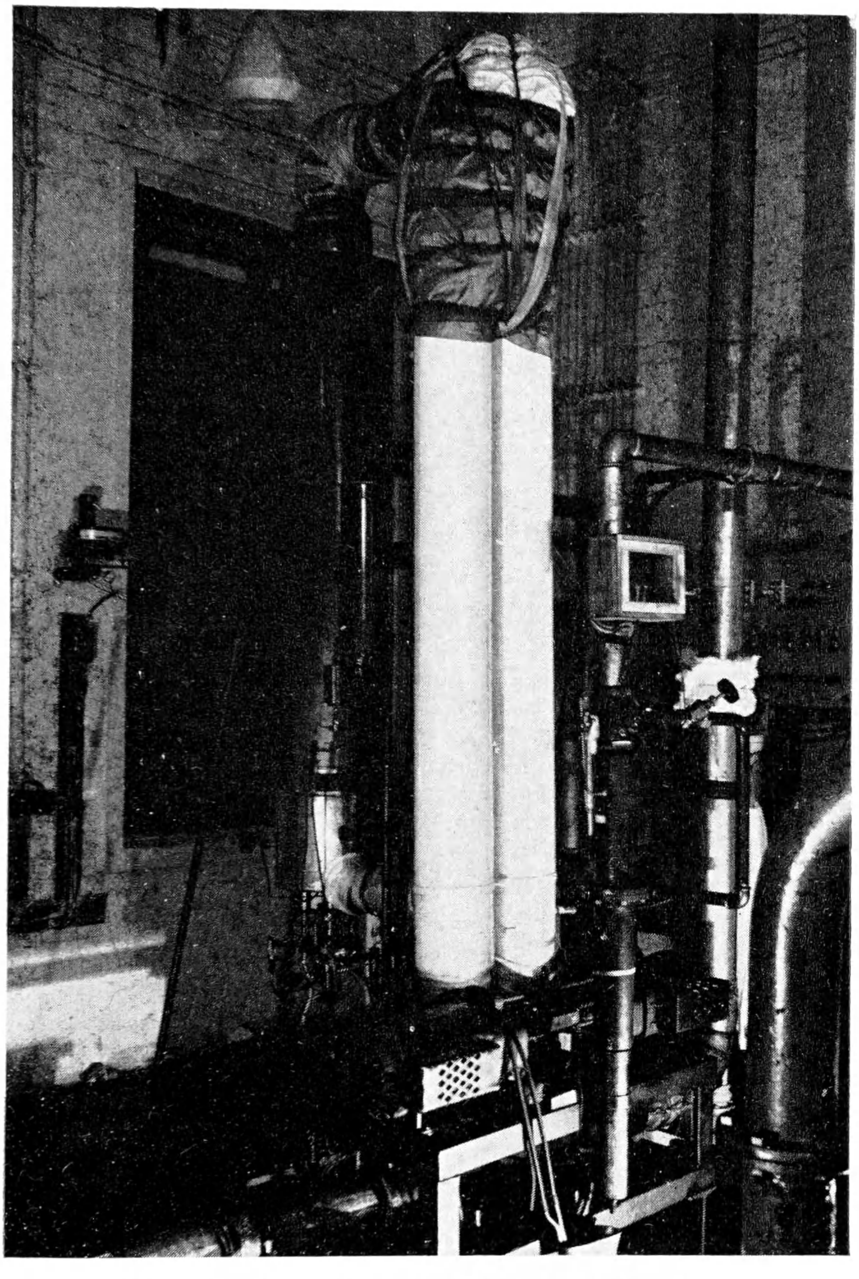

Plate 13. General aspect of the new air liquefier.

PLATE 14. P. L. Kapitza (1924).

the outgoing air stream in the next cycle. Such a cleaning of regenerators from the deposit is due to the fact that air which goes out at a lower pressure has a lower density, and even at a somewhat lower temperature the partial pressure of water and carbonic acid vapours will be sufficient for the impurities to be withdrawn in almost the same amount as that brought in. At the cold end of the regenerator the partial pressure of carbonic acid is small, and its deposit in the tubes of the condenser is insufficient to block them earlier than the regenerators themselves get blocked.

A more serious interfering factor is the blockage of the nozzles of the turbine by carbonic acid. Carbonic acid slowly precipitates on the nozzles, and, if it is taken into account that the cross section of the nozzle is only 1–2 mm^2, it becomes understandable why even a very thin layer of carbonic acid of 0·1 mm can reduce the cross section so much that this may affect appreciably the operation conditions of the detander. The increase in pressure, associated with this factor, can sometimes reach 1–$1^1/_2$ atm. We are combating successfully this undesirable phenomenon in two ways. From the theory of regenerators it is known that the temperature at the cold end in the time interval between switchings does not remain constant, being lower immediately after a switching and considerably higher by the end. These variations attain 28°C.

Since the partial pressure of carbonic acid vapours depends exponentially on temperature, the concentration of carbonic acid increases appreciably in this temperature interval, and carbonic acid coming to the turbo-detander is brought in almost exclusively in that period in which the temperature of the gas leaving the regenerator is the highest. Therefore temperature variations should be smoothed over. For this it is convenient to make use of the adsorption heat of charcoal or silico-gel. A few kilograms of charcoal prove to be sufficient to equalize the temperature.

The difference in the temperature of air at the inlet and outlet of the turbine depends on the degree to which outgoing air is supersaturated with moisture. The turbine operates well when it has a moisture reaching 2–3 per cent or more, but in this case its capacity is somewhat lowered.

In working with a small temperature drop in the turbine, the carbonic acid deposit in the nozzle is small and has no effect during several hours of running, but the output of the plant reduces by few kilograms per hour. We found it hence convenient to work with a large temperature drop, eliminating the solidified carbonic acid from the nozzle every $1^1/_2$–2 hr by means of heating.

The technological and economical characteristics of the plant are to be considered as experimental ones. In constructing the first plant, it was difficult to choose properly the dimensions of all its parts. Intending mainly to check the theoretical considerations of principle, from which a high efficiency of the detander was to be obtained, we tried to get as quickly as possible the experimental material necessary for checking the theory. As already shown, the expected high efficiency of the detander was obtained in practice, and experimental results were in agreement with our theoretical assumptions.

As to the method of liquefying, I think that also here the characteristics of the plant are favourable. The output of our plant is on average 29–30 kg of liquid air per hour (as calculated over 7–8 hr of running, excluding the starting period). About 9·5–10 m^3 of air are used per minute. The pressure drop at the plant is 7 atm. Thus, taking the efficiency of the compressor to be 0·59, we obtain that 1·7 kWh are consumed per 1 kg of liquid air. Of a sevenfold pressure drop only 4 come to the turbine, while the rest are losses in the pipeline and regenerators. It is obvious that these losses can easily be avoided by choosing more suitable tubes, valves and so on. Subtracting these losses in the pipeline, we obtain that 1·3 kWh are to be consumed for liquefaction of 1 kg of air.

If it is taken into account that 0·85 of the capacity of the detander can be used usefully, then the energy consumption should in this case amount to 1·2 kWh per 1 kg. This is anyway better than in high-pressure plants, but still not so well as could be expected theoretically. Apparently, one will succeed in raising the efficiency of the plant by improving the thermal insulation and combating various small losses which in sum total give an appreciable effect. It could be hoped that the power consumed in our plant will be reduced to 1–1·1 kWh per 1 kg of liquid air, but most will be gained when new methods of liquefying are applied for high-capacity plants. Then the efficiency of the detander will increase, and the effect of cold losses will decrease.

The reliability of operation of our plant has been checked already during 9 months of experimental running.

It is interesting to note, for example, that liquid air can be obtained directly from a turbine, without a condenser. Although the efficiency in this case decreases, the operation proceeds normally, even somewhat better, since blockage by carbonic acid is completely absent. The scheme of operation is very simple, because there is no condenser and the flowing out of liquid air should not be regulated. In this case, firstly, it is difficult to separate the liquid from the gas at the outlet of the turbine, and, secondly, the air obtained in such a way is enriched with oxygen up to 40–50 per cent. The liquid air that we obtain in our plant contains oxygen from 24 to 30 per cent.

I think that liquefaction of air at low pressures is of promise, not only owing to the economy of the cycle, which is not decisive in low-capacity plants, but also in view of the simplicity, high reliability and safeness of working at a pressure of 4–5 atm instead of 200. The short starting period—only 18–20 min—makes it very convenient in cases when liquid air should be obtained quickly in small amounts. The absence of decarbonizers, scrubbers and driers undoubtedly simplifies working and confers the plant a compact aspect. At the present, for example, the size of our plant is 5–6 times smaller than that of plants of equivalent output, operating at a high pressure. Evidently, also its cost will be lower.

But, besides the use of the plant for a direct production of liquid air, its main purpose is to compensate simply and efficiently for cold-losses in fractionating air. Therefore the production of liquid air by an efficient cooling cycle is only a part of technical problems associated with an economic fractionation of air. The other part of the problem, which is now being worked out at the Institute, is the distillation of the obtained liquid air and production of liquid oxygen. Here it should be noted that, although an ordinary scheme of fractionation by means of a double column can in principle be applied in working at low pressures, it will apparently prove to be not very convenient. Here other ways should be chosen, which promise more in applying low-pressure cooling cycles. The difference is due mainly to the fact that at low pressures only 5–6 per cent of the treated air is liquefied, whereas at high pressures 15–16 per cent is liquefied. But, on the other hand, air purified from moisture and carbonic acid in the high-pressure cycle is precious, and such air is to be treated in an economical way. Using, in the low-pressure cycle, air which was not previously purified, one can treat it more freely.

And, finally, after this problem is solved a third one arises—production of gaseous oxygen by means of evaporating the obtained liquid air, avoiding cold losses.

9. ON STEREOSCOPIC FILMS

At the request of the editorsof the journal *Mazhinostroenie* (Mechanical Engineering), the scientific collaborators of the Institute of Physical Problems and I have acquainted ourselves today with a system of stereoscopic pictures, invented by S. P. Ivanov.

The basic idea underlying this system is very simple, and this is its great merit. It consists in the fact that a beam of scattered light, reflected from a screen, splits into a large number of bands, passing over the entire hall in such a way that even bands correspond, for example, to the right-hand stereoscopic picture, while odd bands correspond to the left-hand one. The bands alternate at a distance corresponding to that between man's eyes, so that this allows the spectator to find easily such a position in which he may see with his right eye the picture corresponding to this eye, and with his left eye the picture corresponding to the latter. As a result, a stereoscopic effect is obtained.

Invention of an optical system which would allow every spectator at any spot of the hall (up to 2/3 of its interior) to get the effect of stereoscopic sight, is the basic problem solved by Ivanov. The simplicity with which this at first sight difficult problem was solved (introducing a raster of a special geometric form), deserves high praise.

Further, it should be noted that the invention was brought to such a degree of perfection that even those experimental sequences which are presently shown give already aesthetical pleasure.

Looking at the concept of the system we feel that there is every reason to assume that the inventor will succeed in gaining his main object—to give illusion of stereoscopic sight at once to a large audience. The capacity of this audience (200–300 persons) is apparently quite realistic.

Like any new great idea and achievement of technology, this system may invoke many doubts, and its future cannot be foreseen. But we give below the conclusions that we derived.

The stereoscopic cinema shown to us will undoubtedly increase the aesthetical effect of perception of feature films, and, on the basis of what we have seen, it can be stated with a fair certainty that the cinema of the future will be that of three-dimensional pictures.

The objection raised against this invention which we were told about, i.e. that 60–70 per cent of the light is lost, is not valid, because the accommodation of the eye does not follow linear laws, and, when the illumination of an object decreases by a factor of three the visibility decreases considerably less.

П. Л. Капица, О стереоскопическом кино, *Машиностроение*, 12.iii.1940.

Some difference in the sensation of space perceived at different spots of the hall nevertheless cannot deteriorate appreciably the impression produced on the spectator.

It should be taken into account that the invention is merely in its initial stage, and that further work will finally lead to a number of technical improvements (increase in brightness and illusion of space, and so on). But even in the present stage of experimental work it is evident that the basic idea underlying the invention is very sound and valid. It should be recalled that in the history of mankind no invention was brought forward already perfect. It is from this point of view that this invention should be evaluated.

Therefore I think that the work of the inventor Ivanov, who furthered substantially the realization of stereoscopic cinema, should be acknowledged as very important. The ideas underlying this work allow one to hope that this achievement will in due time be considered as one of the greatest achievements in the technique of contemporary cinematography, like sound-pictures.

That is why it is very desirable that S. P. Ivanov should be provided with every opportunity of continuing his work on a broad basis.

Obviously, the next step is to check the invention on a large screen with a large audience. Only in such a way can we find out how much the third dimension is needed in cinema and whether it tires the spectator. Thus, one will be able to estimate how much has the inventor succeeded in achieving his main end—the use of stereocinema for a large audience. Only this check will allow one to give a definitive opinion on this great invention in Soviet cinematography.

10. THE UNITY OF SCIENCE AND ENGINEERING

LIFE in a socialist state is being built and developed on a scientific basis. From the first days of its existence the Soviet Union has therefore shown science great attention and care. As a result we now have a science which is solving most advanced and difficult problems. We also have an industry making most precise and complex mechanisms. But the link between our science and engineering is still weak, and this prevents us from making full use of all the rich reserve of creative forces of our country.

Only with a lively and healthy unity of science and engineering can they assist each other: science opens out before engineering new possibilities, which it invites, but without compulsion, to grasp. With the growth of engineering, science on its part is not only enriched with new technical possibilities, but its thematics are broadened and it becomes more purposeful.

Nowhere is there such soil for the widest application of science as in the U.S.S.R. We can therefore only explain the weak link between science and engineering by the fact that the most effective organizational forms for this interlinking have yet to be found. Doubtless with the present level of our science and engineering, having regard to shortcomings which still persist, much greater technical achievements could be attained. I do not think that this lag is explained solely by the obvious shortcomings which are gradually being outlived, such as a certain "cluttering up" of creative cadres, bureaucracy, disorder in supply, etc. It seems to me that there are other more important organizational shortcomings, the discussion of which has as yet received little attention.

For success in the struggle to master Nature the right strategy and tactics are required. Here, as in battle, the most important things are to deploy the forces correctly along the front and to give clear orders to the fighting men. The command staff of our scientific army not only falls short of the mark, but often the army is in battle with no command. On many fronts of new conquests of our science and engineering we simply do not yet have an organized army, and isolated "guerilla" detachments are in action. Neither is there any concentration of well-chosen forces in definite sectors so as to be able to achieve decisive results quickly. Here one senses, as it were, fear to concentrate the push at definite points, to postpone today the projects, however significant in themselves, which alone bring the tomorrow. And the result is that our creative forces are dispersed over an extended front, and so not one general has won a great battle.

The need for concentrating and unifying the forces is already felt. The latest plan of the Academy of Sciences of the U.S.S.R., though it only covers some

П. Л. Капица, Единение науки и техники, *Правда*, 17.vi.1941.

of our scientific organizations, is endeavouring to concentrate the forces on definite problems. But one plan on paper is very little. It is necessary to inspire the scientific army with the projects and to give it feeling for the significance of victories in particular sectors. It is necessary further to form the fighting units skilfully and to promote Commanders-in-Chief. Life has shown that any influence on the creative workers of science and engineering are only effective with a healthy public opinion to back it up. It is created in the scientific journals and by the press. This most valuable organizational possibility is still insufficiently used.

For example, let us take the general assemblies of the Academy of Sciences. They are well attended. Interesting papers are heard at them, but more often they are professional lectures for increasing education, and not scientific discussions. Almost no debates, no traditions to bring social valuation to individual scientific or technical projects and the research associated with them. Not even an attempt to start a debate. But debates hardly arise on their own, they have to be nurtured. For this it is necessary to select beforehand the most important and keenest issues, and at the same time to choose and prepare opposition speakers. The chairman must steer the debate, formulate the controversial theses, and not allow the debate to wander from the main issues. If the scientific paper is itself not such that is calls forth debate, as, for instance, a simple paper on a scientific discovery, then in any case the meeting should hear the opinion of leading specialists for the right social valuation to be created. Without debates and discussion there is hardly any point in reading papers at public gatherings—it is simpler and cheaper to print them.

It often seems to me that we in the scientific medium even fight against debates and definite valuations. Perhaps this is because we are still not facing up to the pride which is based on the wrong premise that a good scientist cannot be wrong, that an "error" must discredit him. It is as though we forget that "only he who does nothing, does not err". Practically any scientific truth of the present day may be changed or "added-to" tomorrow, for we are in the state of a continuous trend towards finding out the true nature of things. Only by surmounting error after error, revealing the contradictions, are we obtaining ever nearer solutions of problems posed. Errors are not as yet mock-science. Mock-science is the non-recognition of errors. If only for this reason, it is a brake for healthy scientific development.

It is necessary to point out that often our press takes sides in discussing controversial questions in science and engineering. Five years ago an article appeared in the journal *Tekhnika* on the production of oxygen by a centrifuge method. Oxygen is of very great importance in engineering. It is extremely valuable to investigate all the existing methods and their possibilities. In the opinion of a number of scientists the proposed method contained errors which made it impracticable. These objections were not published in the journal. This was instrumental in the possibility of useless work being done on this method. Resources and effort were expended in vain.

Occasionally the discussions will take on a form which cannot at all be regarded as healthy. Two years ago debates were organized in the journal

Under the Banner of Marxism on the constitution of the ether and other fundamental topics in modern physics. The issues were interesting and quite debatable. But, apparently, the discussion lacked any guidance, the main issues were not separated and the controversy began to degenerate so that in some articles the negative personal characteristics of the opponents began to predominate.

The lack of a healthy public opinion on various topics involves not only the possibility of harmful scientific developments to no good purpose, but also interferes with the concentration of the best forces around the central problems.

The lack of discussion is not at all in correspondence with our aims. With us any scientific and technical success is a success for the whole country, and people want to appraise it, not only to understand it. And true public valuation inspires the worker and helps him to create that enthusiasm which is so necessary in creative work. Any notable scientific and technical question is of social significance for us, and therefore it must be discussed and it must be judged.

The organization of the army of creative workers in our industry, it seems to me, is less developed than in the field of science. Whereas our scientists are at any rate gathered in units, the creative workers of engineering are so dispersed throughout our immense industry, that when the problem of mustering them for a shock force is raised, this appears very difficult.

I am again not speaking of those many small battles which lead to technical improvements such as better materials, rationalization of processes, saving of labour, improvement of the quality of production and so forth. This technical progress, though it is often carried out in "partisan" fashion by individual enthusiasts, is of great value and generally speaking it goes on quite well. Most significantly, of course, hereby, in the U.S.S.R., as in no other single country, are engaged the widest circles of technical workers, primarily Stakhanovites, from labourers at the one end of the scale to engineers at the other. But I have in mind other greater battles fought by big divisions, the results of which must place in the hands of our country unprecedented possibilities. The victories may be on the same scale as the invention and introduction of the turbine and Diesel engine in the past, or the underground gasification of coal and oxygen blast in iron and steel in the future.

In these great battles, even with large-scale use of resources and labour, with mobilization of the best creative collective of scientists and engineers persevering with enthusiasm, it is still impossible to guarantee that victory over Nature will be won. But if the victory is won, this signifies a new stage in the development of industry. Achievements of this magnitude will enable us to surpass all. Doubtless the socialist system should be the best organizer of such great engineering victories. Under no capitalistic conditions is it possible to mobilize resources and people so widely for this battle.

So as to use this exceptional opportunity, it is again incumbent on us to learn rapidly to organize our best creative workers into an army in all the leading fields of our engineering and to harmonize their work with our scientists.

From my observations the biggest single organizational shortcoming is that we do not give due significance to questions associated with putting new technical achievements into effect; this work is not usually regarded as creative. In industrial Trust research institutes* and works laboratories conditions have been made for creative work. Yet the very last link—the actual adoption of new ideas—is usually effected by rank and file cadres along with their day-to-day production tasks. But no matter how good a citizen is when he is born, or how long he is educated, any new idea underlying machines or processes is only truly elaborated insofar as it is being practised. Successful "rearing" of new ideas should not be entrusted to the rank and file engineer, foreman, workman, etc., but to a well-selected, highly-skilled creative collective.

I came up against the absence in industry of organized cadres of such types of creative technical workers when it fell on me to put into effect the plants which had been developed by us for producing liquid air and oxygen. These plants, working on new principles of cooling and separation, were only mastered in the laboratory. Industry was confronted with the problem of making this equipment into a reliable production machine which was easy to run.

The greatest difficulties were encountered in organizing the base for their introduction and in selecting the cadres of engineers, technicians, foremen etc., with the initiative to master these machines. Sufficient is known about the promoting of such persons, but they themselves are little known. They are scattered, they are not considered, they do the most diverse jobs. One has to grope to find them, and when one finds them—to get them is not easy. Owing to the continuing lack of understanding of the importance of mastering new possibilities in engineering, our planning executives often oppose the recruitment of the best persons and the arrangement of special conditions for the introduction of "problem" work.

The importance of a healthy base for the introduction of new technical achievements is shown by an example of a foreign firm which through chance circumstances organized a pilot plant with the main task of developing new types of machines. This factory was Brown–Boveri in Switzerland. It rose up in Baden which is a town near Zürich University. By using consultation with great scientists, for instance, Stodola and others, this factory produced very good electrical machines and turbines. However, the surrounding countries raised a customs barrier, and the firm was forced to build independent engineering works in Canada, Germany, France, and so on. Here the technical development of new machines was concentrated in Baden where the firm's best teams were gathered. Thus in Baden arose, probably, one of the most advanced experimental factories.

Of course, the reasons for forming it do not interest us, but it is instructive for us that such successes can be achieved by a factory which fundamentally was only intended to master new types of machines. That one of the foremost factories should actually have arisen in this way is demonstrated, for instance, by the fact that quite recently it successfully produced the first economically operating gas-turbine. And the engineering of the world had been working on this problem for a very long time.

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* At this time many Soviet factories were organized in Trusts under industrial Ministries (*Translator*).

It seems to me that this example shows the force which becomes evident if the best scientific and engineering creative forces are concentrated at one point and directed to the solutions of certain problems, and also if a special base is provided for putting the developmental work into effect.

Therefore, I think, it is an opportune time to raise the issue of combining well-selected collectives of all types of creative workers in each field of industry. Depending on the scale of production, it is necessary for such organizations to include individual workshops, and in large-scale industry even independent plants with specially selected personnel. In this way we shall create much better conditions for introducing into our industry quite new lines of engineering. With thoughtful and flexible guidance of such collectives, doubtless they will open the doors to engineering for all our science, and this will lead to the introduction of the most daring and advanced ideas. It will then be much easier to link the scientific and technical collectives of creative workers. And this will be a great step forward towards narrowing the wide gap at present between theory and practice. You see this division is mainly due to temporary organizational shortcomings. But, in actual fact, as we are coming more and more to see, it is becoming harder to say where pure science ends and applied science begins.

Plate 15. Rutherford.

11. E. RUTHERFORD

Ernest Rutherford (1871–1937), subsequently Lord Rutherford of Nelson, one of the greatest English experimental physicists, famous for his study of atomic structure and radioactive processes. He was born in the village Brightwater (New Zealand), in the family of a small farmer, and finished his university studies in Christchurch. Already in the days of his university studies he was interested in the problems of application of radiowaves for wireless telegraphy, and, in 1892, was one of the first who proved the possibility of carrying out radio transmission. For this research Rutherford obtained a grant, allowing him to continue his scientific research from 1895 at the Cavendish Laboratory of Cambridge University. There, under the guidance of J. J. Thomson, Rutherford investigated the processes of the ionization of gases, and began to take an active interest in the phenomenon of radioactivity, discovered by Becquerel in 1896. After his two-year stay at the Cavendish Laboratory, Rutherford was appointed to a chair of physics at McGill University (Montreal, Canada, 1897–1907), where he together with Soddy founded the theory of radioactive decay, for which he obtained in 1908 a Nobel prize. In 1907 he was appointed to a chair at the University of Manchester (England). Since 1919 he was the director of the Cavendish Laboratory and, for the rest of his life, professor of experimental physics at Cambridge University.

All greatest studies of Rutherford are concentrated about the problems of the atomic nucleus, the theory of which was devised in investigating the phenomena of radioactivity, and represents now a particular field of physics—nuclear physics. Rutherford can undoubtedly be considered as the founder of this branch of science. In the very beginning of his work, Rutherford showed that the emission from a radioactive substance is a complex process, in which the major part of energy is carried by particles (corpuscles). He established that this radiation consists of two parts, the first of which he called β-rays, representing a beam of fast electrons, and the second one α-rays, representing a beam of helium nuclei. In 1900, he discovered the product of the decay of radium called emanation (radon). Together with Soddy, Rutherford in 1902 put forward a theory explaining radioactivity as a spontaneous disintegration of the atoms of some elements, in which the element passes from one position in the periodic system to another one. The revolutionary theory was contrary to the idea of stability of matter, settled since Dalton's times, and was not adopted for a long time. Nowadays it is considered as quite established.

Investigating the scattering of fast particles when they pass through matter, Rutherford discovered that the atom has a positively charged nucleus, and put

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П. Капица, Резерфорд, *Большая Советская Энциклопедия*, 48, 503 (1941).

forward in 1911 an atomic model according to which the atom of any element is like a solar system: in the centre is a positively charged heavy nucleus, surrounded by negative electrons moving in orbits. In 1913 this model was taken as a basis of the atomic and spectral theory of Niels Bohr. At present these concepts are leading ones in physics as well as in chemistry. Finally, in 1919 Rutherford showed that, besides spontaneous decay, an artificial disintegration of matter can be induced in collisions of fast α-particles with atomic nuclei. This was first found for nitrogen atoms, and was later demonstrated on a number of other light elements. Subsequent development of this research by Rutherford's disciples Cockcroft and Walton showed the possibility of disintegrating various elements by bombarding them with protons, i.e. hydrogen nuclei. Thus the old problem of transmutation of elements was solved, and in his last lecture Rutherford called rightly this branch of science "the newer alchemy".

The basic features of the experimental work of Rutherford are the clarity of formulation of the problem and the simplicity of methods of solving it. All basic methods of investigating in the field of radioactive processes were devised by Rutherford and his disciples. His work won general recognition; he was a member of most scientific societies throughout the world and a honorary member of the Academy of Sciences of the U.S.S.R.

12. PROBLEMS OF LIQUID HELIUM

I AM feeling uneasy in starting to present my work on liquid helium. Most of the audience are, of course, used to analytical reasoning which is necessary in every field of scientific work, but I am afraid the problems of physics are alien to many of you.

Research in the field of physics, like any scientific research, can be divided into three parts; the first one is the aim of the investigation, the second one consists of methods for achieving this aim, and the third one of the results obtained and their meaning.

As to the second part, i.e. the methods, in the field of physics they are of great interest for the investigator, and often are the means of success. However, it seems to me that it is very difficult for someone who is not working in a laboratory and in a given field to assess the procedure of research, the technique of the experiment, the methods and accuracy of the measurements, just as it is difficult for someone who likes and understands music but is not a musician to assess difficulties in the technique of a musical performance, although this does not prevent him from enjoying and liking the music, and from being interested in it.

It appears to me that this comment is valid for any kind of creative work.

Therefore, I am intending in my discussion to dwell mainly upon the purpose of each experiment and upon the results obtained. The experimental technique will be mentioned only casually.

The aim of any scientific investigation is defined by the status of science in a given field. Not only the investigator but also people to whom he reports on his work must have a clear idea about this status and problems arising from it. It is here that I encounter the greatest difficulties.

I am afraid that physics represents one of the weakest points in the scientific training of a well-educated man. As a matter of fact, all of us know history well and have read great historians such as Kluchevskii, Tarle and so on. Philosophy and economics are closely connected with our social structure, and we have to deal daily with them. We apprehend easily important concepts of natural sciences such as, for example, Darwinism, and hence are familiar with them. We are also not alien to the problems of technique, since this is connected with the development of industry which is in the focus of public attention. Moreover, technical devices in the form of radios, telephones, cars, and so on are involved in our every-day life. However, we are by far less familiar with leading concepts, of physics and perhaps even less with mathematics.

П. Л. Капица, Проблемы жидкого гелия, *Советская Наука*, **1**, 33 (1941).

Discourse held at the general meeting of the Academy of Sciences of the U.S.S.R. on 28 December 1940.

If we put a question to any educated man about quantum theory or even more concrete topics such as, for example, about the photoeffect, spectra and so on, or if a mathematician enquires what is group theory, probability law and so on, I think that in only one case out of ten an answer showing a general knowledge of such topics can be obtained.

My discourse encounters even more difficulties owing to the fact that the domain of my report is still far from practical life and little known. The point is that in science there are as a rule two kinds of research. Allow me to explain by an analogy the distinction between the two. In exploring our natural resources we can either advance the exploitation of already discovered geological ores, or search for new deposits in nature. Of course, both kinds of research are highly important to us, but we evaluate them differently. When we already know the practical value of the ore exploited, the question of its use is already closely connected with practical life. Then it is easy to assess the importance of a new investigation. However, when research leads to the discovery of new ore deposits whose importance and its value for practical life is difficult to state at once, then, obviously, an estimation of the importance of such work becomes considerably more difficult and can be carried out only long after the discovery.

Such a difference in the character of investigations is found in most fields of scientific research. Let us quote, for example, a great discovery in physics such as induction by Faraday. We know now that without this discovery there would be no electric engines and generators which cover in a dence network over all the terrestrial globe, and are necessary to carry out any technical process. However, after the discovery of induction many decades passed before its practical applications were found, so that Faraday and most of his contemporaries died before realizing the tremendous practical importance of this scientific achievement.

One may point to many similar examples. For instance Hertz, who discovered radiowaves, denied even the possibility of their application in wireless telegraphy. He was quite right from his standpoint, because at that time it was not known that in the upper atmosphere there is a layer reflecting radiowaves and making them bend round the terrestrial globe, owing to which long-distance radio communication is made possible. Röntgen, at the time of the discovery of X-rays, never was able to assume that, besides numerous applications of this important radiation, it would turn out to be almost the sole therapeutic tool for cancer treatment. Therefore if scientific work leads to the discovery of a new and surprising phenomenon in nature, we should not expect to get at once a definitive evaluation of the phenomenon. At a given moment our evaluation can be based only on the unexpectedness of the phenomenon, i.e. on how substantially it contradicts current ideas about the nature of things.

In physics, as in all other sciences, there are a number of branches, which are more or less fully represented by theories, hypotheses and assumptions. The development of science consists in perpetual change, extension and improvement of theories, whereas accurately established facts remain as such. In the course of this development we are continuously approaching the true picture of nature, the understanding of which is necessary to control nature and to master it ever more completely. The most powerful incentive to the development of theories arises as one succeeds in finding unexpected experimental facts which contradict current concepts. If such contradictions become highly acute, then the theory must change and, consequently, advance.

Thus the basic stimulus to the development of physics, like to any other science, appears to be the interpretation of these contradictions. Whence we obtain the basis for the objective estimation of a scientific achievement having no direct application in practice. The discovery of any new phenomenon in nature should be considered to be the more important the more changes it may demand of ideas and theories existing at a given time.

Naturally, a correct understanding of the significance of a scientific piece of work is most necessary to the investigator himself, since this defines the trend of this quest. We think that it is on the basis of these considerations that a good experimentalist has to design his work, taking this design, of course, in a broad sense—as a general trend.

In order that you may get an idea about the aim and importance of the results of our investigations on the properties of liquid helium, I have to present you an outline, albeit the most general one, of theoretical physical concepts with which they are associated.

Within the past fifty years the development of physics turned out to be most affected by two theoretical concepts. The first one is the atomic concept of matter. The development of this concept, in particular as it was associated with thermodynamical laws, yielded a number of very fine generalizations, the most important of which is, of course, the kinetic theory of matter. However, this successful development came in the beginning of our century to one of the most curious blind alleys. From the development of the theoretical generalizations it followed that the balance between matter and radiation was impossible, since it turns out that the entire energy of the thermal motion of atoms must continuously change to radiant energy. This conclusion is well known by physicists, and is usually called the Rayleigh–Jeans paradox.

The history of the development of this contraction is didactic, and so allow me to dwell upon it.

In this case the different attitude of scientists toward theory showed up particularly acutely. You see, there are quite a number of physicists who, owing to their intrinsic conservatism, are inclined to consider theories which they have already well assimilated to be something persevering and constant. It is interesting to note that this attitude toward theory is by far more usual on the continent than in England. Most leading English scientists are as a rule characterized by the fact that they attach importance primarily, to experiment, considering theory only as a subsidiary tool. More than 140 years ago Davy said: "One good experiment is worth more than the ingenuity of a brain like Newton's". This sentence is often cited also nowadays. For example, contemporary scientists such as J. J. Thomson and Rutherford liked to quote it. Of course, this sentence is to be considered as a hyperbola, as a slogan of protest against the idolization of theory. It is curious that the Rayleigh–Jeans paradox has obtained in Germany the name "the Rayleigh–Jeans catastrophe".

By such an epithet it was meant to express the fatal impact on theory of this remarkable contradiction in science.

We know that the result of this catastrophe was extraordinarily fruitful for science. It gave rise to quantum theory. The importance of theoretical concepts implied by this "catastrophe" for the development of contemporary physics should be considered as ranking immediately after the atomic hypothesis. If every catastrophe would lead to such outstanding beneficial consequences as in the case of this one, then we could only wish to have more of such "catastrophes". History shows that science is truly advanced mainly through similar small and great "catastrophes".

As certainly many of us know, Planck was the first to find a way out of the blind alley. The solution was simple, and in its initial stage it seemed to most people to be purely formal.

Transforming somewhat the classical radiation formula and introducing a new constant, Planck showed that the lack of equilibrium between matter and radiation can be obviated. However, the genuine fundamental and universal meaning of this constant, called by the name of Planck, has been understood somewhat later. Physics owes this to Einstein; he was the first to understand the fundamental significance of Planck's discovery, and he gave it a more general physical interpretation called the Einstein law. It appears to me that, by its practical consequences for the development of science, this outstanding work of Einstein's played a considerably more important role than his so popular theory of relativity.

After this work the quantum theory began to develop successfully. The ideas of quantum theory in their most general form may be described as follows. Processes occurring in nature should not be considered to proceed continuously, as was assumed earlier, but as a consistent alternation of elementary states of matter taking part in the processes. We assume now that in nature processes proceed discretely, reminding ourselves of the atomic distribution of mass in matter. Maybe, this does not seem now to us to be so unexpected at it was earlier. As a matter of fact, recently not only theoretically but also experimentally the sign of equality has been put between energy and mass—these two may be transformed into each other. If matter in nature is found only in discrete masses, then the same discreteness is to be expected in energy processes. This, of course, cannot be considered as a proof, but in any case this points out that such a relationship is quite natural.

It is well known that quantum theory in the initial stage of its development was extraordinarily fruitful in studying atoms, owing mainly to Bohr's ideas. The structure and properties of atoms are nowadays known very fully. The processes of radiative emission by the electronic shell of the atom are described extremely accurately and in great detail.

It is mainly the elaboration of atomic physics that led to the great development of quantum theory and to its important generalizations given by Schrödinger, Heisenberg and Dirac.

It is interesting to note that the mathematical apparatus resulting from the quantum description of processes in nature underwent a substantial simplification. If we had, for example, to study a system such as an atom, in which a large number of electrons are moving around the nucleus, by the same methods as those used in celestial mechanics, then this would lead us to greater mathematical difficulties than those we are encountering now.

However, in spite of all these successes, it would be incorrect to think that quantum theory is completed and that it will develop no more. Here a further development can and must follow, and even fundamental changes in our basic concepts are to be expected. If we wish to search for new contradictions in nature, we have to be interested just in those branches of physics in which quantum concepts will undergo the most thorough checking. From this standpoint it seems to me that we have mainly to concentrate ourselves on those branches of physics where quantum phenomena are least investigated and understood.

We owe the creation of quantum theory to the experimental material on the study of atoms. This material is nowadays on the whole completed. The most interesting fields of physics appear to be the following two.

The first one is the branch of atomic nuclei. In the nucleus there are elementary particles spaced so closely that laws derived for large distances between them, as is the case in the atomic shell, are to be expected to turn out here completely unjustified. Hence it may with a high probability be expected that the present-day quantum theory will need a substantial development in order to be adequate for nuclear physics.

The second one is the branch of states of aggregation. Of course, the basic ideas of quantum theory, being of a general character, should cover phenomena occurring in matter surrounding us, where the interaction of atoms and molecules leads to the formation of gases, liquids and solids. But in investigating matter at room temperatures it turns out that the quantum nature of the processes as a rule cannot manifest itself. It appears as though the thermal motion of atoms effaces those features of processes which are imposed by their quantum nature, so that they are imperceptible. The situation is the same as if in a boat rolling on the sea we wished to study the laws of collisions for billiard balls on the billiard table. Obviously, this would be feasible only if the sea were tranquil. In the same way a study of the quantum nature of processes occurring in a state of aggregation is possible only when this nature manifests itself completely, i.e. when the thermal motion of atoms is sufficiently small. Whence it is clear that there is great interest of physics in an investigation of phenomena in matter at very low temperatures—the region of phenomena at the temperature of liquid helium, about which I am going to speak.

To illustrate more concretely this reflection, let me show you a very simple experiment (Fig. 1). Although it represents only a rough illustration of the aforesaid, I think that it will allow those of you who are not used to conceptions of thermal motion to imagine this illustration in a more concrete way. In the projection lamp there is the frame *1* in which a few dozen cycle ball-bearings are put between two parallel glass plates. These balls may be seen on a screen in a considerably magnified form as black discs. The frame is suspended on a series of springs *2* against another frame *3* in such a way that the first one may oscillate in its own plane. By means of a connecting-rod *4* and a crank *5* lying at the axis of a small electromotor *6*, the frame can be brought to perform horizontal oscillations. In this case, as you are seeing, the balls begin to move between the glass plates. Their motion reminds us of that of atoms in thermal excitation. The more rapidly we make the frame oscillate, the more rapidly the balls will move, and the more closely the picture of the motion will remind us of the one at a higher temperature. Now take notice of the following detail in the design of the frame. In its lower part there are a number of partitions *7* separating six pockets out of the common space. If the frame does not oscillate strongly, then the balls contained in each pocket are moving only inside their own pocket, and their motion has no effect on that of the balls in neighbouring pockets. This state is shown in the top drawing. If the frame is moving rapidly, i.e., as if at a high temperature, then we obtain the picture shown in the bottom drawing. The trajectories of the balls are passing all over the frame, and the presence of the partitions forming the cells apparently has no effect on this motion. Assume now that the reversed task is put before us—to detect by the motion of the balls the presence of the pockets whose walls would be made of a transparent, invisible material. Evidently, we would be able to do this only if we studied the motion at small oscillations of the frame, i.e. at a low temperature.

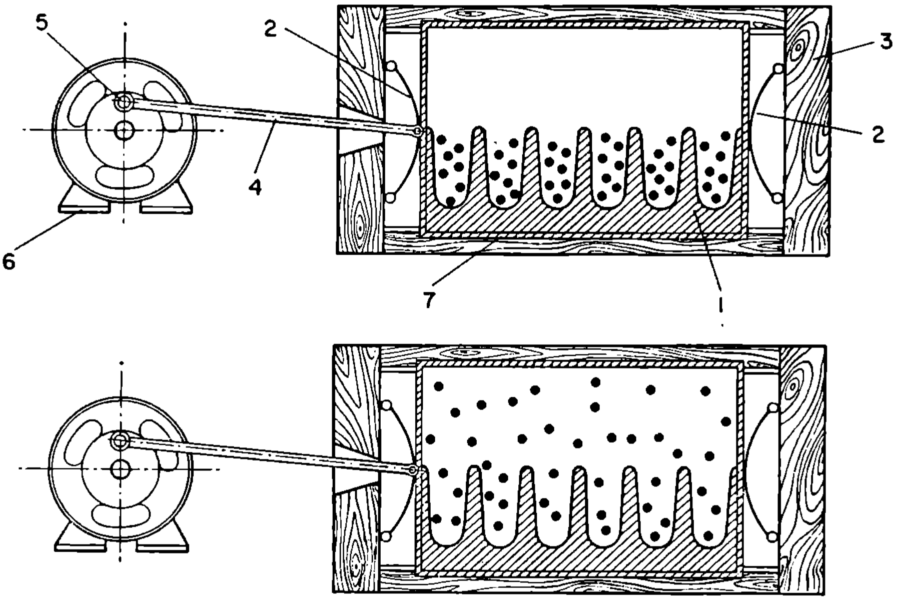

FIG. 1

The situation is the same also in investigating the quantum properties of a state of aggregation. Restrictions imposed by the partitions of the frame in our model upon the motion of the balls remind us somewhat of those imposed by the quantum nature of phenomena upon processes occurring in a state of aggregation of atoms. At a sufficiently low temperature the quantum nature of the interaction between atoms may show up a number of physical phenomena which are not occurring at a higher temperature. It is the interpretation of these phenomena that is of the greatest interest in studying the properties of matter at temperatures close to the absolute zero.

Anticipations that new properties of matter will be discovered at low temperatures are already fulfilled. Even in the very beginning an anomalous behaviour of the heat capacity of bodies and gases was observed at low temperatures, which can be explained by quantum theory, as shown by Debye and Einstein.

According to quantum theory the heat capacity of matter near the absolute zero must approach zero. Indeed, for example in the temperature region 0° to 4° absolute in which we have worked, the heat capacity of most substances is several times less than at room temperature. It is interesting to note that it is owing solely to this property of matter that one can carry out the cooling which is necessary for experimental work at low temperatures. The point is that liquid helium, by means of which the cooling is carried out, has a very low heat of evaporation: it is more than one thousand times less than the heat of evaporation of the same volume of water. Calculations show that with such a small heat of evaporation it would be impossible to cool substances if they preserved the same heat capacity as at the room temperature.

Kamerling Onnes began to investigate physical phenomena in the region of the lowest temperatures, and after great efforts he was the first to succeed in liquefying helium.

Helium was the most difficult gas to liquefy. This is explained by the fact that its atoms are very symmetric and exert very small attractive forces onto one another. At normal pressure its point of liquefaction is only by 4·2° above the absolute zero. In evaporating helium by lowering the pressure, its temperature can be lowered to 0·8° above the absolute zero. A recently deviced method for demagnetization of paramagnetic salts made it possible to carry out a further lowering of the temperature. In such a way one succeeded in appoaching a temperature of only few thousandths of a degree above absolute zero. It may be expected that in the future the absolute zero will be approached even closer, but it is in principle impossible to attain the absolute zero itself.

Of a number of specially interesting physical phenomena observed at low temperatures the most interesting one is perhaps superconductivity—a phenomenon about which you have certainly heard. It turns out that at a certain very low temperature some metals lose completely their electric resistance. A current induced in the metal may circulate arbitrarily long if the metal remains at the low temperature needed.

This phenomenon of the motion of electricity without friction in conductors, as shown by the existing theory, contradicts our usual ideas about the motion of electrons (carriers of electricity in a metal) through a crystal lattice, since this motion should normally proceed with a loss of energy.

Despite a number of very interesting attempts to device a theory of this phenomenon, up to now no one succeeded in it. Physicists are of a unanimous opinion that this phenomenon is due to the quantum nature of phenomena at low temperatures, but the inter-relationship is as yet unexplained.

Some other phenomena at low temperatures which have no analogues at ordinary temperatures and are apparently also connected with their quantum nature turn out to be the properties of helium itself, about which I am intending to speak in more detail.

Liquid helium has a very small specific weight, about seven times less than that of water. It is extremely transparent and, for example, in comparison with water is difficult to make visible. Those who are interested may see it in the next room, where it has been brought for a demonstration. I have not found it necessary to bring it in the auditorium, since liquid helium can be seen only at a small distance.

Investigations of the properties of liquid helium led to the discovery of a number of interesting phenomena. Kamerling Onnes already observed that helium has two states: the first state is a normal one and called helium I. It exists down to the temperature 2·19 absolute. Below this temperature helium exists in another modification; remaining liquid, it goes over to the state called helium II. Helium remains in this modification down to the lowest temperatures so far reached. In a closer consideration helium I appears to be a boiling liquid, since even the light impinging on it makes it boil. In order to protect it from the surrounding heat, the vessel with liquid helium is enveloped by two sets of vacuum shells between which there is liquid air. Without these precautionary measures liquid helium would evaporate in ten minutes or so.

The second state of helium is substantially different from the first one. Helium II is not boiling, and its free surface forms apparently a completely stationary plane. Helium II has a number of extraordinary physical properties. Of these, may be, the most remarkable property is its extremely high thermal conductivity, discovered five years ago by Keesom and his daughter. This phenomenon was detected in measuring the thermal conductivity of helium II in fine tubes, capillaries. At room temperature, substances that are so far known to have the highest thermal conductivity are metals, and of these copper and silver. Many of you probably know by experience that, if one heats an end of a copper bar and holds its other end in his hand, then the hand is likely to be burnt. Now, helium in capillaries proved to have a more than million times higher thermal conductivity than copper. It is quite natural that Keesom called this property, in analogy with the superconductivity of metals, the thermal superconductivity.

Experiments carried out in Canada have shown that liquid helium has a very low viscosity, which is several times lower than that of helium I.

Viscosity is the property of liquid defining its fluidity. If we let run various liquids through the same tube and under the same pressure, then we shall readily see that some of the liquids are running easier than others. The more difficult a liquid is to run through, i.e. the less its fluidity, the greater its viscosity. Consequently, viscosity is so to speak, a measure of the internal friction in flow. From experience, for example, we find that oil has a high viscosity, pitch even higher, whereas water has low viscosity. If a precise experiment were carried out, then we would find that gases also have a viscosity, although a low one. It turned out that the viscosity of liquid helium is about 100 times less than that of water, while in going over from helium I to helium II the viscosity was found to be even lower.

This phenomenon attracted strongly our attention. As may readily be shown, in these properties of liquid helium one can find contradictions with our usual concepts of the mechanism of viscosity and thermal conductivity.

As a matter of fact, how can we imagine the mechanism of thermal conductivity? We assume that heat is the motion of atoms of matter. If a part of a body is heated more than another, then atoms in the former will vibrate more strongly than in the latter. Owing to interaction forces this stronger vibration of the atoms of the more heated part is transferred to the less heated part. The non-uniform energy of vibrations tends to become uniform throughout the body, and this results in the propagation of heat all over the body. This means that thermal conductivity should be considered as the ability of atoms to transfer their vibrations to one another. The higher this ability for transfer, the higher the thermal conductivity of a given substance.

Let us now try to imagine the mechanism producing viscosity. When a liquid is flowing, for example, through a tube, then a layer of the liquid touching the wall is motionless, the next layer already is moving with a certain velocity, the next layer after that is moving with a somewhat larger velocity, and so on. These layers are sliding with friction relative to one another. This friction is due to the fact that the atoms of each layer fail to keep pace with those of the next layer and, owing to the attractive forces, interfere with the motion. As a result there is a loss of energy, which produces the viscosity of the liquid. From this picture it follows that viscosity will be the higher, the stronger the effect of motion of the atoms of a layer on that of the atoms of the next layer, i.e. the easier heat propagates through the body.

Hence with an increase of the thermal conductivity of matter it is logical to expect also an increase in its viscosity, and not the reverse, as is the case of helium. The question arises why is it that the viscosity of helium II decreases as its thermal conductivity tremendously increases.

In order to solve this contradiction we put forward the assumption that the high thermal conductivity observed by Keesom is only an apparent one. As a matter of fact, it is known that there are two mechanisms of heat transfer: the first one is heat transfer from atom to atom, such as that observed in solid bodies and described above, while the second mechanism of heat transfer is convection. Let us assume that you put your hand above a heat source, for example, above a radiator; then you will immediately feel warmth, since the flux of heated air will transfer heat to your hand. Such a heat transfer together with moving fluxes of matter is called convection. But if your hand is put under the radiator, then you will feel no warmth, since the flux of warm air is going up, and the usual heat transfer of air is very small. In a medium with such a poor thermal conductivity as in air, the ordinary heat transfer proceeds only owing to the transfer by convection. Thus it is natural to assume that in helium, whose fluidity is high, the heat transfer by convection will proceed easily. It is by such a mechanism of heat transfer that the high thermal conductivity observed by Keesom might be explained.

Calculations have at once shown that to account for the high thermal conductivity by convection fluxes the viscosity of helium II should be considerably lower than that measured by scientists in Canada.

However, it should be noted here that a low viscosity is very difficult to measure. Theory shows that the true value of the viscosity may be, so to speak, masked by the presence of the so-called turbulent motion in the liquid, i.e. instead of having a smooth flow during the measurements, in reality there is superposed upon it a vortex motion. This vorticity can be shown to deteriorate the results of the measurements in such a way that the obtained value of viscosity may turn out to be many times larger than the true one.

This problem is purely experimental, and I will not consider it at length, since it requires a rather detailed description of the experimental technique.*

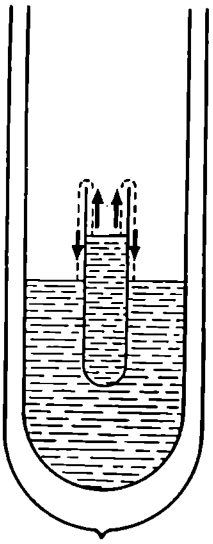

FIG. 2

I will only mention that we have reproduced the measurements of the viscosity of helium taking into account this point of view. We succeeded in designing a viscosimeter with a very fine hole of half a micron (a micron is a thousandth fraction of a millimetre), through which helium was flowing. In such a way we were able to eliminate largely the harmful vortex effect, and so to show the measured viscosity of helium II to be at least a thousand times less than the previously measured value. We also showed the value of viscosity obtained by us to be only the possible upper limit. As a matter of fact, the true value of the viscosity might be arbitrarily less, i.e. even by our fine hole we were not able to prove that we succeeded completely in eliminating the harmful effect of turbulent motion.

We published this work three years ago, and it incited a number of discussions and criticisms. First of all, possible errors in the method of this work were sought for. Now, it is didactic to note an objection raised against us. This criticism was based on another curious property of helium II—its creeping in the form of a film along the walls of the vessel. If the level of helium II in the test-tube shown in Fig. 2 happens to be above that of surrounding helium, then the levels will rather quickly become the same. This effect was studied, and helium was shown to creep easily on the surface in the form of a very thin film. Our experiments were criticized by saying that in my viscosimeter I measured no flow of helium out of the vessel through the hole (in fact, the flow took place by means of creeping) and, hence, an error was made and the values of viscosity obtained by me were lower than the true ones. As a matter of fact, in carrying out the experiment I took into account the possibility of an error due to this phenomenon. However, it is interesting to note that this criticism raised by scientists in America and Canada overlooked the fact that helium can creep in a very fine film of the thickness, according to measurements by Kikoin and Lazarev, less than a hundredth fraction of a micron, but then its viscosity is a million times less than the limit that we obtained. It turned out that the criticism on the high fluidity of helium was based on a phenomenon for whose explanation an even higher fluidity was needed. Based on these observations, we proposed to adopt generally helium II to be an ideally fluid liquid, and in analogy with superconductivity we called this property the superfluidity.

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* The experimental work is presented in detail in an article by the author in *Zhur. Eksp. i Teoret. Fiz.* (1941), see *Collected Papers of P. L. Kapitza*, No. 38; vol. II, p. 625.

One would think that then the possibility arose to explain the anomalous heat transfer of helium II by its superfluidity. However, as experimental data were more thoroughly elaborated quantitively, new difficulties were encountered, upon which I will dwell in more detail.

Calculations have shown that to account for the values of thermal conductivity observed by Keesom the velocity of convection should be assumed to be about 50 m/sec. This is rather a large velocity and, therefore, I wanted to measure it more accurately.

For this purpose a number of experiments were designed, in which a method was elaborated for a more sensitive measurement of large values of the thermal conductivity of helium II than that carried out by Keesom. We succeeded in improving the technique of measuring the differences in temperature, achieving an accuracy to within several millionths of a degree. The description of this technique would divert us from our subject and, hence, I will not talk about it.

Thus we succeeded in observing the heat transfer which was at least twenty times larger than that observed by Keesom. Consequently, the convection velocity necessary to account for this thermal conductivity should be not 50 m/sec, but of the order of magnitude of a thousand m/sec. Obviously, one cannot assume the existence of such velocities in convection fluxes. It cannot be assumed that helium is moving in a capillary with a velocity which exceeds that of a bullet. It turned out that energy sources for such powerful convection fluxes are not available.

It appeared that our results led to even more fundamental difficulties than it seemed at first sight, if the mechanism of heat transfer by convection was ruled out. If we recall the ordinary mechanism of thermal conductivity, which we have earlier described as a transfer of thermal motion from atom to atom, it can be shown that also in this case we are facing a basic contradiction.

Let us return for a while to this picture of thermal conductivity. Assume that a layer of atoms is rapidly heated and that the atoms in it are vibrating more intensely than in the neighbouring layer. These oscillations will be transferred from the first layer to the other, and thus there will appear a heat wave propagating from the heated spot all over the body. It has been shown that the propagation of such a wave cannot be more rapid than that of elastic oscillations in the body, i.e. of sound. The velocity of sound in helium II was studied and found to be 230 m/sec, whereas velocities which we obtained from thermal measurements were several times larger, which contradicts the conditions of such a mechanism of heat transfer.

Attempts to find a way out of these contradictions took us about a year. The question arose how to investigate further the mechanism of this heat transfer without a guiding idea, since our results on the whole contradicted all known theoretical concepts.

We had to grope our way, try various physical factors under the effect of which the thermal conductivity might change. We investigated the effect of pressure, gravitational force, time and so forth on heat transfer in helium II. The results proved to be negative; the thermal conductivity did not change, i.e. it remained high.

Finally, a completely unexpected observation defined a new trend of our work. It turned out that the pulsations of pressure quite accidentally transferred from the laboratory network of the helium pipeline to helium in the capillary strongly changed the thermal conductivity. Although the pulsations were very small they lowered the thermal conductivity of helium II by a factor of several times ten. The question arose how these small pulsations of pressure could affect so strongly the thermal conductivity of helium.

The most natural explanation was the following. It is known that liquid helium II is a relatively easily compressible liquid (about hundred times easier compressible than water). Owing to this property the pressure pulsation compressing the liquid was able to produce helium fluxes in the capillary where the thermal conductivity was investigated. We assumed that just these fluxes affect the thermal conductivity. In order to check the validity of this explanation, we had to design experiments in which the thermal conductivity of helium flowing through a capillary would be measured. When this was done it turned out that in helium II running through the capillary the thermal conductivity was indeed a hundred and even a thousand times lower. These experiments also showed that in so far as heat was flowing through helium in the capillary, helium was running out easily. By this fact a connection was established between the fluxes of liquid helium and its ability to transfer heat, and this was the clue to further investigations.

Indeed, if the helium fluxes affect the thermal conductivity, then it is also possible that the heat transfer gives rise to the fluxes. The question arose at once how to detect experimentally the helium fluxes in a fine capillary with a cross-section of only 0·5 mm. The problem was difficult, but it could be expected that these fluxes would come out at the free end of the capillary, and that there they could be observed. For this purpose a device schematically shown in Fig. 3 was designed.

The glass capillary *1* was put horizontally. One of its ends, bent upward, was connected to the glass bulb *2*, in which a heater was put. Opposite the free end of the capillary there was the wing *4* in the form of a disc suspended on the yoke *3*. The yoke was suspended on the long glass stick *5* by means of a fine quartz filament. The yoke and the bulb with the capillary were situated deeply below the free surface of helium II. If during heating of helium in the bulb the liquid would come out of the end of the capillary, then this liquid hitting the wing would exercise a pressure on it so that the wing must be deflected.

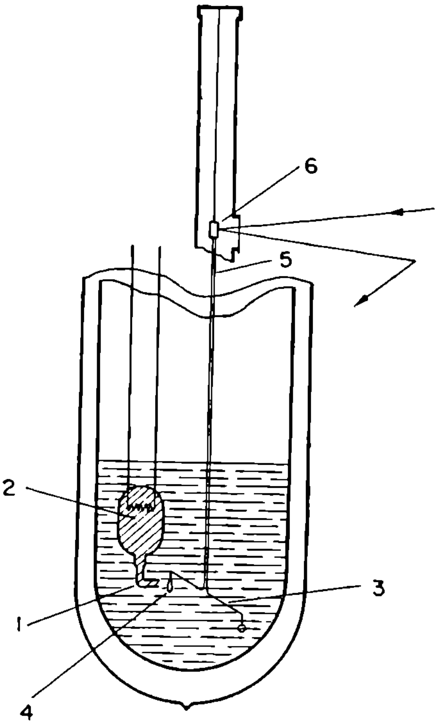

FIG. 3

The force of the pressure could be measured by the torsion of the quartz filament, which in turn can be determined by the deflection of a beam from the mirror *6* fixed to the glass stick *5*.

The experiment has shown that even at the slightest heating of helium in the bulb the liquid indeed comes out of the end of the capillary, and that this liquid exerts an easily observable pressure on the wing.

The character of the helium flux from the outlet of the capillary could be established by means of the following simple experiment. The disc of the wing *4* had a small diameter, somewhat larger than that of the aperture of the capillary. The experiment has shown that if the disc was put almost next to the outlet of the capillary or at a large distance exceeding by a factor of 15 the diameter of the aperture, the same pressure can be obtained. However, if the disc is put at a large distance it should be placed exactly in the direction of the capillary, otherwise helium exercises no pressure on the disc. This experiment shows that the helium flux comes out of the capillary in the form of a well defined jet. A detailed quantitative analysis of this phenomenon and its dependence on the load will not be discussed here. We shall only point out that the velocity of running helium (it amounted to 5–6 cm/sec) could be determined by measuring the magnitude of the pressure of the helium flux on the wing.

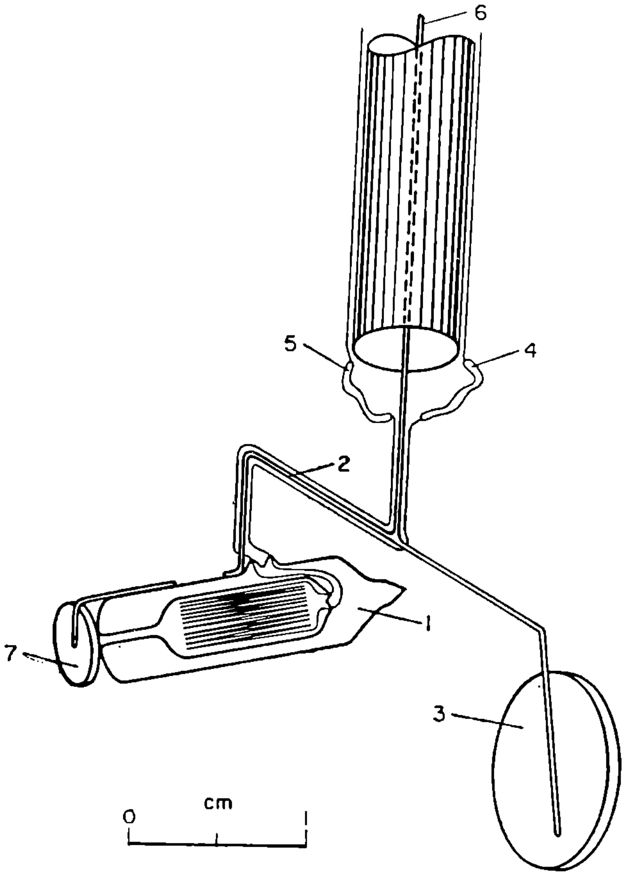

FIG. 4. The small suspended bulb with the heater for studying the reactive force of the jet.

Since helium is running continuously out of the capillary in the form of a jet, the question arises how helium running out of the bulb is replenished, and in which way it is coming back. Namely, it must in some way get again into the bulb, since the quantity of liquid helium in the bulb was not decreasing. Evidently, the sole way is the same capillary, and one would think that, if the wing is placed sufficiently closely to the outlet, incoming helium should also exert a pressure on the wing. However, the experiment has shown that, even if the wing is approached as closely as possible to the outlet of the capillary, still the jet exerts on the wing the same pressure as at a long distance. This means that helium is creeping into the capillary in a very fine layer surrounding the outlet.

In order to investigate more accurately the way in which helium is creeping into the capillary we designed experiments based on the following reasoning. The outgoing helium stream must exert a reaction on the capillary, i.e. induce a force in the direction opposite to the motion of the stream. It can be shown that this force is equal to the pressure on the wing. Liquid helium flowing into a capillary also can exert a pressure on it. By observing and measuring the force of this pressure we can elucidate more accurately in which way is helium coming through the capillary into the bulb. In order to observe this force the device shown in Fig. 4 was designed.

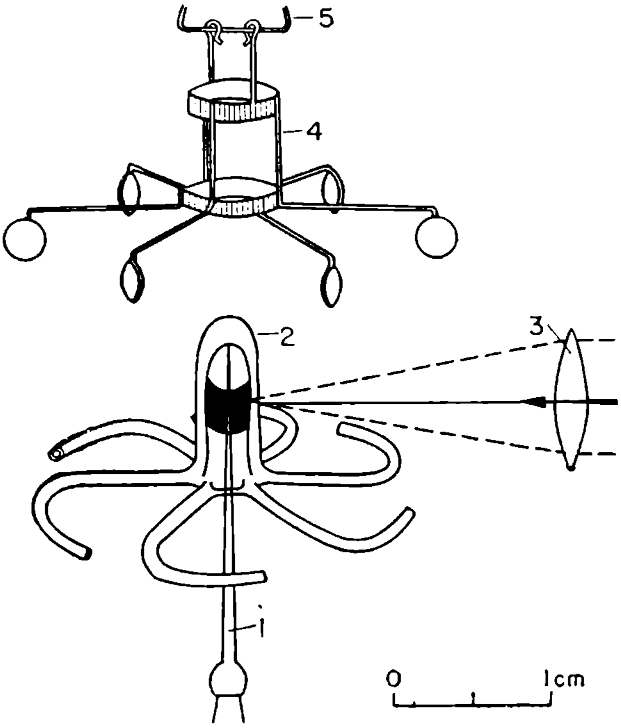

FIG. 5

A very small bulb *1* with a capillary was constructed. Its weight amounted to not more than 0·25 g. The bulb was suspended (instead of the wing shown in Fig. 3) on the same yoke *2* against the glass stick *6*. Then it had to be counterbalanced by the disc *3*. The heating of the helium in the bulb was carried out by a current transmitted by conductors in the form of very fine silver strips *4* and *5*.

Experiments with such a suspended bulb have shown that there is the action of the stream on the capillary and that it is of the same magnitude and character as the pressure of the stream on the disc. In order to show this more accurately, the disc 7 was fixed to the bulb so that the pressure exerted by helium on the disc should balance the force of the stream action on the bulb, whereas the remaining force should be attributed to incoming helium. In the experiment this last force was almost imperceptible and, hence, we came to the conclusion that helium was flowing into the bulb without exerting a pressure on it. The sole possibility for helium to come into the capillary in such a way is by creeping on the surface in a very thin layer.

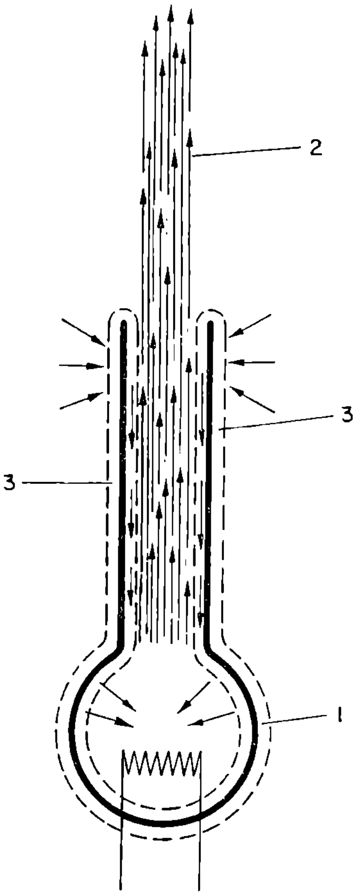

FIG. 6

This phenomenon is so interesting and striking, and we shall be able to show it to you on a simplified device specially designed for demonstration. This device is drawn in Fig. 5. We called it the spider. It consists of two glass cowls *2* soldered at the bottom, to which six capillaries in the form of spider legs are fixed. The glass cowl rests on the very sharp needle *1* and, thus, it can freely rotate. Of course, the entire spider is dipped into liquid helium II. If helium in the vessel *2* is heated by means of the light beam *3*, then it will stream in a jet out of each leg, and the force of reaction of these jets will, as you will see, make the spider rotate.

If now using the loop *5* we put on the spider two very light rings *4* to which six discs (according to the number of the capillaries) are fixed by fine filaments in such a way that each of the discs is screening the aperture of each capillary

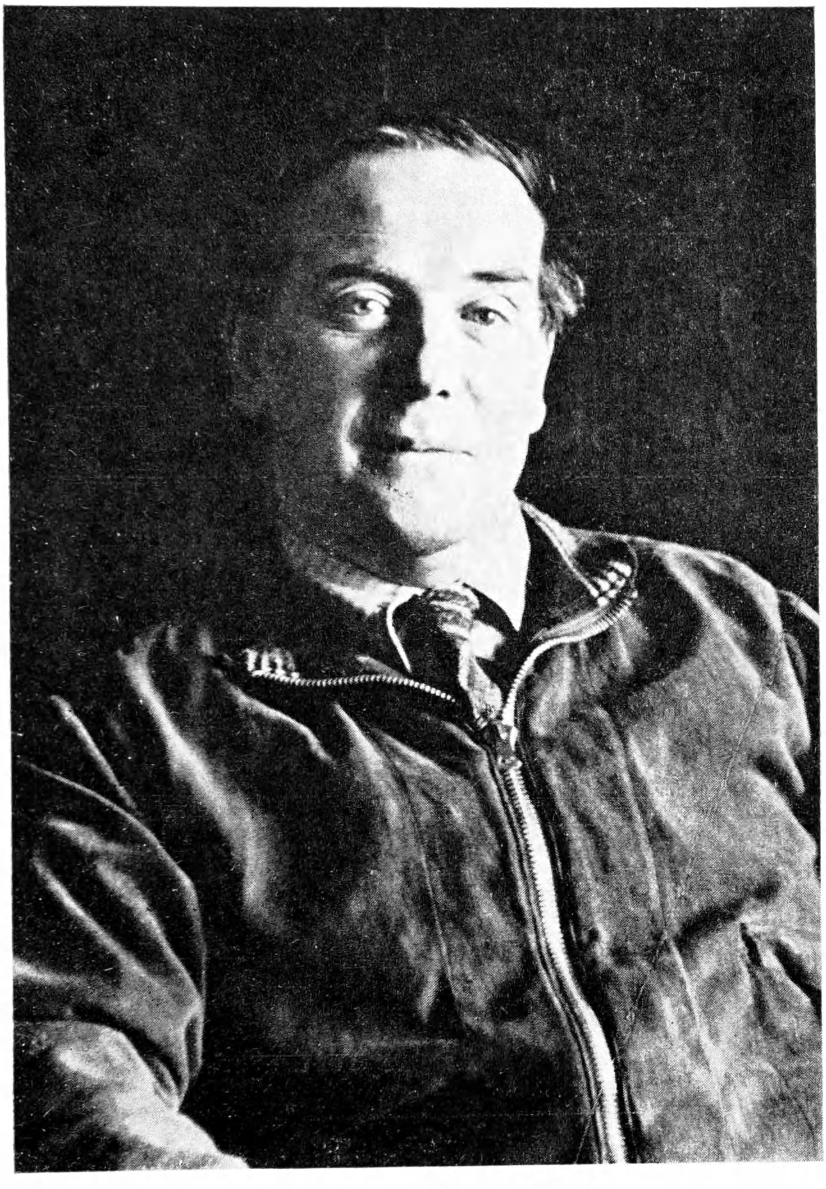

Plate 16. P. L. Kapitza (1935)

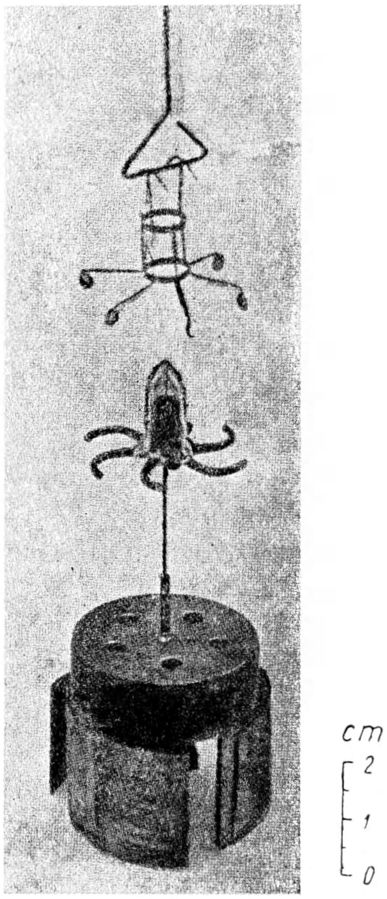

PLATE 17. A photograph of the "spider".

at a distance of 1–2 mm, then we shall see that in such a state the spider will no longer rotate as helium is heated by the light beam.

This little device is shown photographically in Plate 17. It is very small (the diameter of the spider amounts to only 3·5 cm), hence it is difficult to show it at once to all of you. But after the discourse those who are interested will be able to see its rotation by approaching more closely to the table. However, in order to demonstrate the existence of this rotation a film was produced of the experiment, and all of you will see on the screen the rotating spider many times magnified.

On the basis of this experiment we obtained the picture of the motion of liquid helium in the capillary induced by the heat flow. The scheme of this motion is shown in Fig. 6. Helium is heated in the bulb *1*; in the capillary a flux is formed and is running out in the form of the stream *2*. Incoming helium is creeping along the wall of the capillary in the opposite direction in the form of a thin surface film *3*, while in the bulb helium is going over from the surface again to free helium. Thus there is a phenomenon of the creeping of helium on a surface, which is very similar to that already mentioned in the beginning. It accounts for the levelling of helium from the vessel, as shown in Fig. 2. It should be noted that in both cases such a motion of helium is possible only if liquid helium II in its flowing behaves as a fluid having no viscosity.

Now, having the picture of the motion of helium in a capillary due to the heat flow and, as we have seen, established purely experimentally, we were able to derive conclusions from our study leading to an explanation of the extremely high thermal conductivity of helium II.

We have reasons to assume that helium moving on the surface in a thin film differs in its physical state from helium flowing in the opposite direction in the central part of the capillary. We assume that, owing to molecular forces from the wall of the capillary, helium is here in a somewhat different energy state or, speaking in terms of thermodynamics, its thermal function differs from that of free helium. This assumption is, apparently, quite sufficient to explain the large helium heat transfer observed in the capillary. Such a picture gives us grounds for assuming helium to absorb heat in flowing along the inner surface of the capillary into the bulb and, leaving the surface, going over to a free state. It is this process that produces the impression of the extraordinarily high thermal conductivity. Let us elucidate it by an example. If we want to carry out cooling and use for this purpose a stream of cold water at 0° or ice at the same temperature, then we shall see that in the second case, on account of the latent heat of melting, the cooling proceeds more efficiently than in using simply water. The cooling taking place in the heated bulb reminds us just of the cooling by melting ice. Helium coming in over the surface leaves the wall and goes over to another energy state, absorbing heat produced here by a heater.

Based on this picture one can show that the heat transfer becomes qualitatively as well as quantitatively completely explainable, and that there is no thermal superconductivity in helium II.

A further verification of our picture of the heat transfer in helium II in a capillary was carried out by measuring its thermal conductivity not in a fine tube but in free space. In such a way we measured the thermal conductivity under conditions in which the possibility of heat transfer by the motion of the film was ruled out. In these experiments, for example, a glass cylinder with a heater and thermometer inside was used. It was suspended freely in helium II on very fine conductors. The films of helium II, obviously, could creep from cold to warmer spots only along these conductors, but because these conductors had a very small surface only a very small part of the heat could be transferred by the motion of the film along their surface. Hence the major part of the heat should pass through the bulk of liquid helium. Measuring in these experiments the thermal conductivity of free helium, we obtained for this quantity, as was to be expected according to our picture, a normal value, i.e., its magnitude proved to be not higher than in the case of helium I, and was about a thousand times lower than that of copper.

Here I will allow myself to conclude the description of our experiments on liquid helium.

I wish to apologize once again for my describing them very schematically, speaking only about what could illustrate the development of our idea. But it seems to me that even from this very general description one can get an impression of the progress of the study of this interesting problem. You have seen how the observed contradiction of the high thermal conductivity and low viscosity in liquid helium led to experiments which have demonstrated that this viscosity is not only low but practically negligible, and how we assumed helium II to be superfluid and its thermal conductivity to proceed by convection. Such a picture led again to a blind alley, because it is nevertheless inadequate for explaining the high thermal conductivity of helium II. In order to find a way out, it was necessary to observe the motion of helium in a capillary. This was carried out in a number of experiments upon the description of which I dwelled in more detail. In order to explain the high thermal conductivity of helium, on the basis of the obtained picture of motion we put forward the assumption that the thermal function of helium in thin films differs from that of free helium. The hypothesis turned out to be fruitful, and proceeding from it one was able to predict that no anomalies would appear in the thermal conductivity of helium in a free state in the absence of the surface phenomenon.

But I would lead you into error if you would conclude from the aforesaid that the problems of liquid helium are completely solved and that the question is settled. Further analysis reveals many more contradictions and vaguenesses, and a lot of interesting work is still before us. A detailed consideration of these problems would lead us very far afield, and I will point out only a few of these.

Boundary layers which, as we have seen, play such an important role in phenomena occurring in liquid helium II, incite a number of problems to be investigated. For example, problems about the mechanism of the helium flow in thin films on the surface, and about the possible velocities of this flow. One would think that the surface layer of liquid helium II taking part in the counterflow should be assumed, according to a number of general theoretical considerations, to be very thin. However, then it turns out that the velocities of this flow would be very large—of the order of magnitude of 200 m/sec. A more detailed analysis shows that in principle no physical laws are against the existence of such large velocities in a thin film, but we can adopt their existence only after having confirmed it experimentally.

There remains to be elucidated also the question whether the superfluidity is the property of helium II only in its surface layers, or whether it is the property of the bulk of helium. Analysis of experimental data gives so far no unique answer to this question and, on the contrary, leads to a number of very interesting contradictions liable to experimental investigation.

One may further point to quite a number of unsolved problems.

But it is already now interesting to discuss the possible significance which the data obtained may have for the development of contemporary theoretical physics. As we have already shown, the superfluidity of helium II represents a phenomenon very similar to superconductivity. In both cases at a temperature close to absolute zero, where the quantum nature of phenomena is expected to manifest itself, the flow of electricity as well as of matter begins to proceed without losses. It is to be expected that both phenomena might be explained by a single theory, a so far unexplained special aspect of quantum processes in the state of aggregation.

Superconductivity is a phenomenon in which the carriers of electricity—electrons—can flow without friction through a crystal lattice. Superfluidity is a phenomenon in which atoms can orderly move with respect to one another also without friction. Theoreticians are searching for quantum relations accounting for the possibility of such a motion without friction. It is natural to think they will more easily succeed in solving the problem by studying interactions of identical atoms of liquid helium II than those of electrons with atoms constituting the crystal lattice of metal.

Here the presentation of our work could be concluded if, quite unexpectedly for me, an idea were not proposed about a practical application of the high fluidity of liquid helium. I would like to speak about it not because I am sure it can be carried out in practice, but only to illustrate the fact that every phenomenon discovered in nature reveals new possibilities which in some way or other will invariably be used in our practical life. These regions of application may turn out to be quite unexpected and bearing on fields alien to the investigator, in which he is not versed and could not think about in carrying out his work. You will, probably, as well as I be astonished that there is a certain possibility to apply liquid helium in designing airplanes. I was told about this challenging idea by Prof. L. G. Loitsyanskii. This idea is as yet far from being carried out, and may incite a number of objections, but is so challenging that one should speak about it.

It concerns the streamline test of the airplane wings and fuselage. At present, engineers have to make use of very large and expensive aerodynamic tubes, where airplanes are often tested in their natural size. As is known, one cannot apply reduced airplane models, because similarity theory on which experimental work with models is based is here completely inapplicable. In scaling down in aerodynamic tubes there is necessary the same reduction of the so-called kinematical viscosity of the surrounding medium. This kinematical viscosity is the quotient of the division of the viscosity by the density of the medium. In order to reduce it one tried to raise the air pressure in aerodynamic tubes, since in this case the air density increased, the viscosity became constant and, consequently, the kinematical viscosity decreased. This proved to be very expensive and complicated. It is interesting that the kinematical viscosity of nearly all fluid media is larger than or little different from that of air. The single exception is liquid helium. The proposal of Prof. L. G. Loitsyanskii is just to study the aerodynamical properties of airplane models in a flux of liquid helium. This idea is theoretically correct, experimentally challenging and, maybe, promising. In any case, before putting it in practice much work is still needed on the properties of liquid helium itself as well as on the technique for the realization of this idea.

At this moment we find interesting the unexpectedness of this possibility of applying the new phenomenon.

I have no doubts that in the future many more such unexpected and promising possibilities will be found. However, it should not be expected that they will be found by the investigator himself—it would be an unfair and useless burden for him. These ideas should and will appear as a result of collaboration and interest in interdependent research of people with creative imaginativeness who are trying to develop various branches of science and technology. The necessary condition for the most rapid application of new scientific achievements is to raise interest in science and make it more accessible to a wide circle of workers in our country. Science needs for its inculcation also to be popularized. In our country it is the Academy of Sciences that should best cope with this task.

13. SCIENCE AND WAR

WAR demands maximum effort of the belligerents—not only of the army but of the whole organism of the country. Our industry, transport and agriculture must give their utmost; for the greater our output of agricultural produce, munitions and armaments, the swifter our advance to final victory.

War demands unusual effort by creative, scientific thinkers. For instance, factories must simultaneously increase their output and cut down the number of their workers, at the same time that they lose some of their sources of raw materials. Thus, to raise labour productivity by improving technology and the process of production acquires particular importance. Hence, the exceptional value and need of inventive work.

The need to relieve the burden of the transport system makes it essential to harness local resources to serve industry. This in turn necessitates a search for new sources of raw materials or, in the absence of such sources, a search for substitutes. In this field the principal task falls to science.

Finally, armaments too must be constantly improved. The creation of new types of weapons and the perfection of old ones confront science with a whole series of urgent questions that must be answered.

That is why, both in our country and in the countries allied to ours, the war has faced scientific workers with many problems urgently requiring solution.

The whole Soviet people, including the scientists, understands well enough that only by straining ourselves to the utmost can we drive out the hated invaders with least damage to our country. We understand that the struggle now going on is one of life or death, and that the yoke of Fascism would not only turn the collective farmer into a serf under a German landlord, but would deprive the Soviet scientist of his freedom for creative work and of the joy of serving his country and world culture. It is this realization that powerfully spurs our scientists onward.

In peacetime it may occasionally have been possible to censure our scientists for not being invariably able to direct their work into channels most useful for the practical needs of our national economy. It may have been possible to reproach them for that academic abstraction, a hangover of the past, which sometimes marked the scientific work of some researchers. But now the threat to their freedom and their desire to save their country has inspired our scientists and directed their efforts toward the solution of present-day tasks. They are all striving urgently to supply answers to the questions put to them by the war.

Thus, several of our mathematicians who before the war occupied themselves

П. Капица, Наука и война, *Красная Звезда*, 26.xi.1942.
P. L. KAPITZA, Science and war, *Science*, **95**, 396 (1942).

with profound and abstruse problems of mathematical theory, that held meaning only for a small number of contemporary persons, have now successfully centred their attention on immediate problems. One such problem is the application of the conclusions of the modern mathematical theory of probability to the calculation of trajectories of projectiles in flight, thereby improving the accuracy of gunfire.

The wartime work of Soviet scientists may be classified by trends. Some are engaged on problems of broad national-economic significance—the study of sources of raw materials, of substitutes, of utilization of waste products, etc. This work is of particular importance now when we have temporarily lost some of our raw material sources and have had to shift our principal industries far to the east. A special commission of the Academy of Sciences, working at Sverdlovsk, has already achieved important results in this field.

War conditions limit the utilization of some of our resources and prevent importation of many raw materials. To make up the deficiencies with substitute materials is a huge task, mainly handled by our chemists. As an example I cite balsam salve. It is well known that importation of Peruvian balsam—an important component of the Vishnevski curative salves familiar to thousands of our wounded men—involves many difficulties. At present, one of the institutes of the Academy of Sciences is experimenting with a synthetic substitute of which there is no shortage. There is reason to believe its curative properties are not inferior to those of Peruvian balsam.

In another field, our scientists are giving counsel to industry to help it bring its productive forces into full play, to improve the technology of production, to increase output and make more rational use of resources of raw materials. This work comprises no small part of the efforts of our scientists, who frequently pay consultative visits to factories. The scope and multiformity of this work are so great that its full significance is often difficult to appreciate.

Lastly, our scientists are directly participating in the improvement of armaments and defense methods. Stalin has said that our tanks and airplanes are not inferior to the tanks and planes of the enemy. This fact by itself is extremely significant. It is well known that our aircraft industry is still quite young, having been practically non-existent before the Revolution. At first we naturally had to study and copy the achievements of the west in this domain. But we rapidly passed beyond the copying stage, and our aircraft industry long since stepped out on the path of independent, creative enterprise.

To say that Soviet scientists played a decisive role in the success of our aviation is not an exaggeration. After all, the qualities of a modern airplane depend almost entirely on the ability to calculate the profile of the wings and fuselage so that in flight the craft will present least resitance to the air. Experience shows that the slightest deviation from theoretically calculated profiles can considerably reduce flying qualities. These calculations are among the most exacting and interesting of modern aerodynamics. The theoretical work done in this regard by the group of young Soviet scientists produced by the school of Zhukovski and Chaplygin has in many respects left Western European researchers considerably behind. Without these achievements our planes, which have enabled our airmen to beat off the enemy's best squadrons so successfully, could never have existed.

Our scientists are well aware that in producing armaments they must not rest on their laurels for a single moment, that only constant improvement of our weapons brings the hour of final victory nearer and reduces the number of sacrifices that must be made before it is achieved. Boundless possibilities open up for scientific thought in this domain. Our science is making use of these possibilities, enhancing the defensive strength of our country. It would be difficult to enumerate all the major and minor undertakings launched in this field and which are already yielding results, even if it were possible to talk about them now.

It is interesting to note that there is not a single field of scientific thought that can not be of value in modern warfare. There is no speciality whose represensatives can not put their attainments at their country's service. Physiologists are confronted with such new problems as improving the sight of observers and studying the effects of certain diets and drugs. A peaceful study such as the deciphering of cuneiform inscriptions proved to be of service when it was shown in the last war that experts in cuneiform and hieroglyphic writing were best equipped to decode secret enemy ciphers. Our botanists are working out rules for camouflage, taking account of seasonal changes in vegetation. Our historians are successfully helping fight the unprincipled pseudo-scientific propaganda of the fascists.

The struggle now being waged is giving an exceptional stimulus to scientific thought. The strain and tension caused by war are exposing the weak spots in our economy, technique and organization, showing the points where the state must first of all be assisted, and clearly formulating the demands which society makes on science. Although the war demands great sacrifice and causes much devastation, the upsurge of scientific work which is taking place in our country, and which must develop still more, will not lose its value after the war. The new war-revealed possibilities for unified development of our technique and economy will continue evident in the post-war period as well. History proves that this is true.

It is generall known, for instance, that when the Continental blockade cut France off from the colonies which had supplied it with cane sugar, Napoleon ordered his scientists to search for new sugar sources. Systematic work by French scientists led to the discovery of the method for extracting sugar from sugar beets, now the most widely used method. During the war of 1914–1918, the process of nitrogen fixation was introduced and used on a large scale in Germany, which had suffered an acute nitrate shortage. The inventor Haber had not been able to find an industrial application for his discovery before the war. Germany was saved from speedy defeat and, after the war, the synthesis of ammonia spread throughout the world, serving as a basis for obtaining one of the best agricultural fertilizers.

In the course of the present war a number of similar achievements may undoubtedly be expected. For obvious reasons it is impossible to indulge in a concrete discussion of the scientific work being carried on in the Soviet Union at the present time. It is already clear, however, that the war will lead to further improvements of our air fleet, will make for better motors, will teach us to achieve high productivity in industry with less workers, and will bring our theoretical, creative thought closer to the practical needs of the country. The sum total of the achievements of our Soviet land will have a tremendous bearing on the development of scientific thought serving world civilization.

14. ON THE 25TH ANNIVERSARY OF THE ESTABLISHMENT OF THE SOVIET STATE

On the 7th of November the Soviet Union marks the 25th anniversary of the new social order. In connection with this memorable date it appears but natural to sum up the achievements attained both in the cultural and the economic development of the country. Social development is perhaps most prominent in that friendly and powerful union of the nations of the USSR with which they, notwithstanding all hardships, have concerted their efforts to carry out their heroic and stubborn struggle with the hitlerite hordes. As to economic progress, this is perhaps most manifest in the first-class technical equipment of the Red Army.

In our journal we want to mark the memorable 25th anniversary of the November Revolution by discussing from among the numerous attainments of the Soviet Union notably those which are connected with the progress of natural science in the USSR during the last years.

The main idea underlying the development of natural science in our country, contrary to what was the case in prerevolutionary Russia, is that science is at present the main foundation, intended to support the general economic and cultural growth of the country. In other words, science is recognized not only as an essential element, but even as one of the main bulwarks of a socialist state. That is why, since the very first days of Soviet rule, the government has devoted so much care and attention to science, striving to turn the course of its progress along such channels as would contribute most efficiently of all to the all-sided development of the country's culture.

In that inheritance which the new order received from the tzarist regime, the field of science had certainly a definite value. There existed in former Russia a number of large scientific centres which gave the country a vast number of outstanding scientists. These centres were principally grouped with the largest universities of the country. The share contributed by them to world culture is connected with the names of such scholars of world-wide renown as Euler, Lobatchevski, Tchebyshev, Liapunov, Setchenov, Timiriazev, Metchnikov, Pavlov, Mendeleiev, Stoletov, Lebedev, Golitzin, Tchernov, Joukovski, Karpinski and others. The Soviet power began cultivating its science not only utilizing the cultural inheritance of the past, but also extending and deepening it, all the while seeking for novel organizational form sfor planned scientific work and striving to create new scientific centres.

P. L. Kapitza, *J. Phys. USSR*, 6, 233 (1942).

Within the elapsed 25 years this work is far from being completed. The ultimate forms of scientific work have not yet become finally crystallized; yet, the results so far achieved already permit of a more rational utilization of scientific forces to serve the socialist state. As an illustration, we might point to the fact that in the present war, Soviet aviation and several other branches of military engineering have, since the very outbreak of hostilities, proved their qualitative superiority over the technique of the adversary. This has been made possible only by our military engineering finding itself a powerful stronghold in a number of outstanding scientific research works, grounded in their turn on a deep and strong theoretical scientific foundation.

For example, the supremacy of our pursuit and fighting planes is certainly due to the research of this school of Soviet specialists in aerodynamics—this school being founded by Professor Joukovski and continued by Professor Tchaplygin as well as by a number of their followers. The largest institute of aerodynamics of the country, by the request of the pupils of N. E. Joukovski, bears the name of that eminent scholar.

Since the very days when Soviet science began to be organized on a novel basis, a significant tendency arose, consisting in the initiation of a number of large-scale research institutes devoted to mathematics, physics, mechanics, chemistry, etc. These institutes became severed from the universities and represented self-consistent scientific institutions. Concomitantly, there were created a series of military, industrial, engineering and agricultural institutes, connected with the People's Commissariats controlling the respective branches of industry. Being in their work under the general influence of the most prominent scientific institutions of the country, these institutes had, as their foremost goal, the task of serving our industry and national economy as a whole. At first these institutes were apt to develop rather spontaneously, but then with time the necessity began to be felt of making their structure more consistent and their activities better coordinated. And so, as a single organizing and guiding centre, the Academy of Sciences of the USSR began to take an ever more prominent part.

The Academy of Sciences of the USSR is an institution which retains unmolested a number of erstwhile principles, and traditions, as conceived by Peter the First and as upheld at its opening in 1725. The Academy—now as before—is the gathering place of the most eminent scientists of the country who are elected members of the Academy by secret ballot.

The Academy of Sciences continues to be considered as the supreme scientific body of the country. But the functions and the aims of this institution tend to expand gradually. While in former times the Academy resembled an elect learned society, it is at present a widely disseminated large-scale institution comprising a number of scientific laboratories and institutions with more than 3700 research workers. The Academy represents, in the main, the country's theoretical thought both in the field of the sciences and the arts.

The Academy's scientific institutions are guided and their work is coordinated by the Council of the Academy, consisting solely of members of the Academy.

Territorially the growth of scientific institutions began from such large centres like Leningrad and Moscow, but, as scientific work began spreading far and wide and new contingents of research workers came to the fore, there appeared possibilities for creating scientific research institutes in such cities of the Union as Sverdlovsk, Tbilisi, Kharkov, Kiev, etc. and other big towns. Thus, the next stage in the development of science in the USSR was connected with the necessity of a certain decentralization in the guidance of scientific institutions. Academies of Sciences belonging to various republics began to be set up, each of them being from the structural viewpoint a kind of replica of the Academy of Sciences of the USSR. The number of such republican academies is steadily growing; there already exist now a Ukrainian Academy, a White-Russian Academy and a Georgian Academy. Doubtless, this process will continue, bringing about new academies in other republics of the Union. The scientific institutions of the Academy of the USSR are called upon to effect general guidance in respect of all scientific activities in the USSR and also to coordinate scientific development with the requirements of cultural and economic growth in the USSR. It would be wrong to say that this influence has already become all-pervading and that a well-proportioned scheme of scientific development has already been fully achieved in all parts. The task of coordinating scientific guidance in respect of the whole economics of the country is so new and comprises such a vast number of most various problems that it could not even be fully accomplished within the 25 years of Soviet rule. But we may be bold to think that the basic concepts regarding organizational structure have already become well outlined. With every year they take ever more definite shape, and the organization of science as it is to be seen to date exercises a manifest directionalizing and invigorating influence promoting the growth of the country's culture and the development of science itself.

Problems of planning are intrinsically bound up with the organization of science. These problems are very important and very novel; therefore there is but small wonder that they should have been so eagerly discussed from the first.

Most acute were perhaps the discussions of the following contradiction—the state requires high organization of scientific work: thus, it calls for the study of such natural phenomena which are the most inherently connected with processes of production; it does not encourage parallel research, while, on the other hand, it urges the need of cooperation between different branches of science in solving complex problems. Again, the state stresses the need of concerted effort in the search for the most important kinds of mineral wealth, etc. In contrast to such a unified and coordinated scheme of governmental control, some advanced the opinion that this might hamper the freedom of the individuality of research workers, since, it was argued, the greatest achievements of science and human thought in general cannot square with a preliminarily worked out schedule. But, little by little such forms of rational planning were found which provided both the requirements of the state and the free display of individuality. It is evident that only these forms of planning were capable of yielding the maximum results for the state. The planning of scientific research, as it operates now, is considerably facilitated by the centralization of the main scientific institutions of the country in a single organization—the Academy of Sciences, which body while immediately stimulating and promoting the work of individual scientists, strives at the same time to provide for the prime interests of the state. In its support and guidance to men of learning the Academy in no way thwarts the creative freedom of research workers and takes due account not only of the specific features of each field of knowledge but also of the individual aspirations and peculiarities of separate eminent scientists.

Parallel with this work of organizing science, great attention is being devoted to the training of new contingents of scientific workers. It should be noted that this was rather a weak spot during the first years after the revolution, when the rapid reconstruction of the national economy required the best contigents of the youth to be actively engaged in industrial operations in the field. But, as industry began to be reconstructed and was accordingly adequately staffed, deeper insight into industrial operations came into being, new seeking in the field of technique became manifest, the broad masses turned again to science and a large number of most capable young men and women began once more to give their preference to university education. Within the last years the preparation of scientific workers is making considerable headway, the creation of a vastly ramified network of postgraduate study departments and scholarships, the introduction of scientific degrees contributing greatly to the cause in question. The training of new contingents of postgraduate students under the guidance of the leading scientists of the country is held to be one of the most vital and important problems in all leading scientific research institutes.

Considering itself to be an integral part of world science, Soviet science has ever been striving to establish the closest cooperation with leading men of science throughout the world.

Young scientists are being trained in the same spirit. The actual materialization of the principle of world-wide cooperation can be seen in the fact that a number of international congresses were held in the USSR (the Physiological Congress, the Geological Congress and others). It is also evidenced by the publication of Soviet scientific works in foreign languages (it is indeed for this purpose that the present journal* is published, just as are *Acta Physicochimica*, *Comptes Rendus de l'Académie des Sciences de l'URSS*, and other periodicals of the same nature). Our acknowledgment of world science finds itself expression in preparations for celebrating the coming jubilees of Newton and Galileo. And it is peculiar of the spirit of our science that Galileo is still held in the USSR to be a great scientist of world repute despite the fact that his countrymen are fighting in the ranks of fascist troops that have invaded our fatherland.

From among various existing branches of science, our journal touches most closely upon physics. The development of physics in our country progresses at such a rate that we are capable of printing only the most prominent papers in foreign languages. All in all, we count no less than ten large-scale physical institutes, which embrace all the most important branches of that science, such as problems of the atomic nucleus, low temperatures, the property of the solid state, magnetism, semiconductors, etc. Our readers are of course well aware of the trends and results of the respective works to which the pages of our journal are mainly devoted.

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* That is, the *Journal of Physics of the U.S.S.R.*

As the country grows economically, as we pass from the stage of imitating foreign designs of machines and industrial processes to the stage of creating our own original ones, with every year greater and greater claims are being made upon science. Day in, day out, the broadest masses begin to appreciate ever more the significance and high purport of science. The work of scientists is always more and more fully reflected in the life and development of the country. Within the last years the achievements of Soviet scientists have found recognition in the government initiating a number of annual prizes bearing the name of Stalin, who shows great personal interest, towards the work of our scientists and the development of science in our country.

These numerous and highly remunerative prizes mark the most outstanding researches in all fields of science as well as the most important inventions. The sum total of these prizes amounts annually to several score million roubles.

For the first time these prizes were distributed in 1941. In physics they were awarded to P. L. Kapitza (first prize) and A. Alikhanov jointly with A. Alikhanian (second prize). Notwithstanding the hardships of the war the prizes were awarded this year too, the total sum as well as the number of prizes being increased. In 1942 the first prizes in physics were awarded to A. Joffe and jointly to L. M. Mandelstam and N. Papaleksi, while the second prize went to V. Kusnetzoff.

Every year grow the funds allocated by the state to the requirements of science. As things are now, the sum-total of funds spent on science in diverse fields exceeds 1 milliard roubles. The Academy of Sciences within the year 1941 alone received 150 million roubles. This is the maximum amount ever allotted by anyone country to scientific pursuits.

We—Soviet scientists—are proud of the work which has fallen to our share within the 25 years of Soviet rule and we see in our work our part of assistance rendered to the country in its task of building-up socialism. Within the last year we have been obliged to strain all our forces to help our fatherland in its heroic struggle with the hitlerite hordes. In rendering our help to the cause of victory we all know well that further growth of science and culture both in the USSR and in the whole world will only then be possible when we destroy hitlerism and its tyranny. We are also proud of being able to collaborate with progressive democratic countries in their efforts to promote world culture and we rejoice in the feeling that we too are contributing our share towards the treasury of civilization for the happiness of mankind at always higher stages of evolution.

15. WE FIGHT FOR FREEDOM

I HIGHLY value the honour of receiving the Faraday Medal. Faraday's name for any research physicist is associated with the highest reminder of those ideals to which every physicist strives in his work.

The Faraday Medal, according to the statute of the Institution of Electrical Engineers, is awarded not for all scientific works, but only for those which have a direct influence on the development of contemporary electrical engineering. This requiring of a link between science and engineering rings very significantly for us, Soviet scientists, since in our work we set ourselves the chief task to master Nature by our scientific work so as to raise the culture of our country and help the cultural growth of all mankind.

For me this medal has also a certain personal value. Having worked 13 years in Great Britain, at the centre of its scientific activity—in Cambridge—I learned to value deeply the distinctiveness and force of British scientific thought, its clear and simple and therefore strong approach to solving the most complex scientific problems, to value the victories of "common sense" in which British physics is so rich, and which so brightly shone in the works of my teacher—Rutherford—a scientist most close to Faraday in the character and strength of his genius.

For the recognition of the work done by me, I feel flattered, but I also feel that my services have been of no such order of significance as the services of friends and teachers who have received this medal before—Parsons, Thomson, Rutherford, Bragg. I believe that my receiving this medal is due to the circumstances of the time. In February this year, when the award was announced in our press, I was delivering lectures to the command staff of the Red Army.

I see in this medal a symbol of the uniting of the efforts of two freedom-loving and democratic countries who are fighting for freedom, for true culture and for that science which, using the collaboration of scientists of all the world, serves a common purpose—to raise the well-being of mankind. This contradicts Hitler's obscurantism which acknowledges only one germanic science. Such inordinate praise of "German genius", as alone capable of directing world science, is quite absurd and contradicts common sense and objective fact. We naturally cannot help but value the contribution to world science of German scientists before the hitlerization of Germany, but to speak of its monopoly of significance is, of course, laughable. As a matter of interest I may remark that the Faraday Medal has itself for many years been awarded to scientists of

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П. Капица, Мы боремся за свободу, *Труд*, 28.x.1942.

Speech on the occasion of receiving the Faraday Medal 26 Oct. 1942 in the U.S.S.R. Society for Cultural Relations with Foreign Countries.

different countries—amongst the laureates, besides Britishers, there are Frenchmen, Italians and Americans. But there are no German scientists. This does not speak on behalf of German scientific genius as universal.

Finally, the success of a scientist's work in our times is inseparable from the environment and co-operation in which his work proceeds. Therefore, by accepting this medal, I want to see that the recognition applies not only to me, but also to my comrades—scientists of the Soviet Union—for their contribution to science on behalf of world culture.

16. REPORT ON THE ORGANIZATION OF SCIENTIFIC WORK AT THE INSTITUTE FOR PHYSICAL PROBLEMS OF THE ACADEMY OF SCIENCES OF THE U.S.S.R.

ACADEMICIAN A. F. IOFFE, the chairman of our meeting, in his opening address has linked my present speech with the decoration given to employees of our institute, and I cannot but say a few words as regards this.

We are proud of such an attitude of our Government towards us as a collective that succeeded in achieving the results which won the recognition. We are the more happy in that we have deserved this recognition during the war which our people are waging for the defence of our Motherland. When the peace sets in, our decorations and medals will remind us of that small contribution which we attempted to give to this struggle of millions for freedom and happiness.

The basic theme of my report with which the Presidium of the Academy of Sciences entrusted me is the organization of scientific work at the Institute for Physical Problems.

Since I returned to work in the Soviet Union, the problem of organization of science in general, and particularly of scientific work at my institute, has interested me very much. I was familiar with the organization of science and scientific work abroad. I was for a number of years the director of an institute in the focus of English scientific thought—at Cambridge. On the basis of this experience I have felt that the forms of organization of scientific work which are adopted in the West are not completely applicable to our work. It appeared to me that we have to search for our own forms of organization of scientific work at the institute and even more of our science as a whole.

This is mainly due to the fact that in our socialist country an outstanding place is assigned to science. Of course, in other countries it is also well known and generally adopted that science plays an important role in the development of culture and technology of the country. But in our country science is recognized as one of the primary bases of the development of culture, and is assumed to be a guiding force in the development of our technology and national economy. Therefore the organization of science should here be more purposeful than in capitalist countries where it is of rather a fortuitous and spontaneous character. With us the connection between science and life should become closer and fuller.

П. Л. Капица, Доклад об организации научной работы в Институте Физических Проблем Ададемии Наук СССР, *Бестник Академии Наук СССР*, 6, 75 (1943).

Report read on the 18th May 1943 at a meeting of the Presidium of the Academy of Sciences of the U.S.S.R. and general organizations of the Academy of Sciences of the U.S.S.R.

The problems of the organization of science are particularly important for us who work at the Academy of Sciences of the Soviet Union.

In speaking about the organization of scientific work at our institute I will try to give a picture of those general principles of organization of science from which we started, and thereupon I will give an account of what we in fact carried out.

Allow me to make some preliminary remarks.

I am going to speak mainly on the organization at the institute in the pre-war time. Since in the war-time we changed the form of the institute so as to adapt it to the requirements of the war-time, I will say a few words about this at the close of my report. But this, of course, should be considered as a temporary stage in the existence of our institute. The structure of the institute as it was in the pre-war time is what is of constant interest for us. The sounder this structure, the easier it can be adapted to war conditions whenever they arise.

I wish also to remind you that our institute is a new one; it was built only 7–8 years ago. Although I came here as more or less already an accomplished scientist, it was nevertheless difficult to create the institute without having a school and collaboration. Therefore the institute advanced much more slowly than if it had been detached from some other institute and starting from this continued to develop and increase on its own. Additional difficulties in the selection of staff were associated with the particular features of our work dealing with strong magnetic fields and low temperatures—a field of scientific work which was at that time little developed in the U.S.S.R. In the first years we were engaged in the formation and training of the basic personnel of the scientific and auxiliary staff of the institute. Only after the formation of the working core, the institute could begin to make progress and increase normally. This explains why our institute is developed less than it will be in due time.

With these remarks allow me to pass on to the report.

A question which I put to myself from the very beginning was: what were to be the aims and purposes of an Institute of the Academy of Sciences? In solving this problem I, of course, bore in mind an institute of physics or, in general, an institute devoted to studies in a field of natural sciences; the purposes and organization of any institute engaged in other fields of science would surely be different, hence I make in advance a reservation against too broad a generalization of the theses I am going to develop.

Furthermore, I underline that it is the organization of any institute of the Academy of Sciences that is in question. And the Academy of Sciences represents the major manpower of Soviet science. It is from my standpoint called upon to guide ideologically our scientific work from the top to the bottom, and to determine a sound trend of our science. It is at least expected to do this, but if it has not as yet succeeded in fulfilling this, then at any rate it should tend toward these ends. Each individual institute of the Academy of Sciences should carry on the same politics, i.e. should tend to have a decisive influence on science in the field in which it is engaged and to further it.

Therefore, the primary task which an institute of the Academy of Sciences should put before itself is fundamental research. The fundamental studies are those bearing on the fundamental phenomena necessary for gaining a profound insight into nature. The aim of fundamental studies is to provide the necessary knowledge in order that nature may be transformed so that it may serve human beings in their cultural development. Hence the choice of basic themes to be studied by the institute, the choice of fields in which its work should proceed, appears to be of extraordinary importance. The trend of the institute should correspond to the one in the development of science which at a given moment seems to be the most promising and which at a given stage of science, taking into account available methods, can be furthered most rapidly and fruitfully.

I think that in the field of physics there are three such basic trends: investigations in the field of low temperatures, those in the field of the atomic nucleus and, finally, those in the field of solid state. I cannot now go deeply into the reasons for which I find these trends most important, and it is possible that a number of colleague-physicists will not agree with me. Our institute is investigating phenomena occurring at low temperatures near the absolute zero. It should be noted that of late this trend appears to be one of those trends in physics which exhibit the most rapid advance, and one can expect from it many new and fundamental discoveries.

The scientific work is carried out at our institute by a small number of leading scientific workers. This makes the work at the institute more purposeful and concentrated on a few major themes. Nothing is so harmful for the scientific work at an institute as an obstruction by minor themes digressing from basic aims and purposes. The basic themes to be studied at the institute are devised by a small number of its scientific workers—three–four scientists—and prove to be clear of purpose.

After the choice of a general trend of the work the next important problem is the selection of the scientific staff. In fundamental research only a man of a great creative talent can score marked successes. Such men are not frequent in science, but at any rate they cannot be numerous, the same as there cannot be in a country a lot of great writers, composers and artists. To make up for it we should ensure them such conditions of work that their scientific abilities may be used most fully and rationally for the development of our fundamental studies. Therefore the core of the institute must necessarily be formed of only a few very thoroughly selected scientists. This core should be devoted completely to scientific work. The institute should be so organized and such working conditions should be established in it that the scientific workers may devote not less than 80 per cent of their time to scientific work, their time being by not more than 20 per cent spent for fulfilling social and other functions. Only under such conditions will the scientific workers become able to work on their own in the laboratory. Only when one works independently, by one's own hands, and carries out experiments, even if often the most routine part of these, can one achieve genuine results in science. One cannot do good research by somebody else's hands. A man who spends only a half hour or so for guiding a scientific work cannot be a great scientist. In any case, I did not see nor hear of any great scientist who worked in such a way, and I think this is impossible. I am convinced that when even a very great scientist stops working by himself in the laboratory he not only hinders his development but is no longer a scientist. These principles are very important, but they refer only to peace time; in war-time one has to act differently.

It is of particular importance to implant these principles in scientists-to-be. For this purpose I am trying to put their work in a somewhat rigid organizational frame. For instance, the scientific worker is not allowed to be engaged simultaneously in several themes, especially when he is just beginning to make his way. When the scientific worker becomes more advanced he, perhaps, will be able as an infrequent exception to carry out simultaneously two to three studies, but he must begin with only one.

The next organizational mode important for successful work is that every scientific worker should work in the laboratory for a limited number of hours. Work by fits is harmful—it overstrains one and lowers one's creative ability. For instance, in our laboratory it is adopted that all work in laboratory should stop after six o'clock in the evening. A scientific worker should go to his home, consider his work, read, learn, and have a rest. In exceptional cases, with the permission of the assistant director, one can work up to eight o'clock in the evening. Night-work is allowed only with the permission of the director, and is to be justified by technical requirements due to special conditions of the experiment. Such are the working conditions at our institute.

An institute which in the capacity of its scientific staff and efficiency is able to become the centre of fundamental research may nevertheless become an isolated unit closed on itself and not satisfy those requirements which we have put before ourselves in the beginning, i.e. to exert the most effective influence on science and culture of the country.

How can an institute show its influence on the development of up-to-date science of the country, how can it connect itself with other hearths of the scientific thought of the country? There are several ways for this. I shall list only the major ones.

First of all, the institute has for this purpose to make use of those advantages which it must possess as an institute of the Academy of Sciences. These advantages consist in rich and modern facilities and in the selection of an adequate staff, which makes it possible to perform certain studies that would be too difficult to carry out at other institutes. For instance, at our institute the presence of a special device for the production of large amounts of liquid helium reveals exceptional possibilities for carrying out experiments in a range of low temperatures which otherwise cannot be reached. Enjoying this advantage our institute makes it possible for the scientific workers of other institutes to carry out here their studies in the range of low temperatures. These studies are usually not leading ones, and sometimes even stand aside of the basic themes of the institute.

The coming of colleagues from other institutes to ours is usually organized as follows. The colleague who wishes to work at our institute is to report at our scientific meeting or seminar on the experiment he would like to carry out. The matter is then discussed, and if it turns out that the proposal is of actual scientific interest, while the author proves to be adequately qualified, one provides him with the opportunity to perform the work. In order that the fundamental studies of our institute should not be deranged, the number of these side-investigations is kept low and as a rule is not allowed to exceed two to three.

Those who would like to come to work at our institute in the Soviet Union proved so far to be more numerous than our possibilities to set them up. This shows that the institute is a vanguard one, for only in such a case are foreign scientific institutions interested in the work of the leading academic institute and wish to make contact with it.

The fact that we have here always several scientific workers from other scientific institutions allowed us to make a sort of active contact with the outside scientific world. Going away from us after finishing their work the scientific workers report to their institutes not only on the experience derived from the work they had carried out but also on other studies that have been carried out at our institute, and so our experience is being spread ever more widely at other scientific institutions of our country. Thus, through them an active contact with other institutions is established, and we in turn get to know what is being studied there. The active contact is the most efficient one. To take advantage of it proves to be a good method of furthering the development of science in our country.

In perspective it is necessary to establish such an active contact also with foreign scientists. In the first years of our institute foreign scientific workers used to come here. But within the past few years the political situation got so complicated that, although those who desired it came here, a contact with foreign countries in general was disturbed, so that this aspect of our contact with foreign scientists can be considered only in prospect. But, of course, such a contact should be considered as a normal and sound condition of the work of any academic institute, since science the world over makes a single indissoluble whole. If an academic institute aspires at a leading position, then not only scientific workers from its own country but also those from foreign countries are expected to wish to work at it. This will be an objective evidence that up-to-date fundamental research is being carried out at the institute.

There is one more sphere of influence on our culture and science on the part of leading academic institutes. The sphere is the training of scientific manpower.

None other that the institute can train its future staff, and it has to form very carefully and progressively this staff out of juniors. Therefore the institute for post-graduate studentship that we built should in every possible way be encouraged and supported. But there are some difficulties which I wish to discuss.

The first such difficulty is the selection of the post-graduate students. The point is that the inter-relation between scientific institutions and universities in our country is unsatisfactory in a number of cases. I think that this is a serious disadvantage in our organization. Our best and greatest scientists passed into institutions for scientific research. The leadership of universities thus rests mainly on teachers of a pedagogic mould for whom research does not represent the major part of their activity. It must frankly be said that most of our professors cannot be considered as leading scientists of our country. Their system of educating the students and what they demand of them as a rule do not tend to distinguish students of the greatest creative talent. Therefore creative inclinations of our students are insufficiently developed at our universities. In attending post-graduate examinations I usually noted that professors estimate most highly not those students who most of all understand the subject but the ones who most of all know data. But science needs those who in the first place understand the subject. Hence it is very difficult to choose from universities post-graduate students on the basis of their marks. In order to choose properly the promising post-graduate students one has to observe them during a certain time interval when they are engaged in work such that they may manifest their intrinsic vein and their ability of thinking independently. I think that it was the lack of inter-relation between universities and institutes for scientific research that led to the fact that the selection of young scientific manpower is now much more inadequate than at my time when the main scientific work was being carried out at universities. I remember the period when academician A. F. Ioffe held the chair of physics at the Polytechnical Institute of Leningrad. I think it was not by chance that just at that time his group of researchers yielded a number of young scientists who advanced well (four of these already became academicians). Undoubtedly, there are at our universities also at present many promising and gifted students, but the sieve with which we try to sift them for scientific work has such holes that they slip through and do not get into institutes for scientific research. If we wish to begin to select the most gifted scientists we have to think seriously how to find a form of inter-relation between our institutes for scientific research and universities such that students of the greatest creative talent may be revealed and trained.

Therefore we started to search for new modes of selection of post-graduate students out of students at universities. This mode of selection, which we have applied at our institute, began to develop only within 2–3 years before the war, and it is for the present difficult to predict its results. It consists in the following. Taking advantage of the fact that we dispose of liquid helium for low-temperature experiments in amounts larger than those available in the total of all cooling laboratories in the world as a whole, we had the opportunity to organize at our institute a practical training for all students of the Faculty of Physics of the University of Moscow. Of course, this practical training was at first organized only for the best students, but within the past two years all students of the Faculty of Physics without exception passed this practical training, and each of these carried out 2–3 laboratory works with liquid helium. From the standpoint of cryogenic institutes this is a great luxury because, for instance, at the Leyden and other laboratories the work with liquid helium is also nowadays inaccessible even for scientists, whereas here every student of the University of Moscow was given the possibility to carry out experiments such as, for example, on the properties of superconductors, on magnetic phenomena at temperatures close to the absolute zero, and so on. Naturally, the university welcomed this opportunity and sent willingly the students to our institute. In the course of the practical training the following system was established: the best students who proved to be excellent at the work imposed by the practical training were noted and, if they wished, were allowed to carry out more than the 3 works they had to do originally. In this case the scientific workers leading the practical training had discussions with the students, and referred the best of them to come and talk with me. Thus we made it possible to distinguish the most gifted students, to approach them beginning from the 3rd–4th course and to follow them. Furthermore, we engaged the best of these as probationers at our institute. They took part in this activity already when they were doing research as junior laboratory assistants, helping our scientific workers in their experiments, making records, setting simple operations, and so on. The selection of post-graduate students was made already out of the probationer staff not only on the basis of the examinations but taking into account also the quality of their work at the institute. Such a selection of young scientists allows one to cover a larger circle of students and to rule out any fortuity in selecting. At this stage our trial was interrupted by the war. But if we could continue it, it was to develop as follows: after finishing their post-graduate studies and obtaining the candidate's degree these young scientists were to go to other scientific institutions and spread the scientific experience of our institute. Furthermore, it could be expected that one out of ten or one out of fifteen of those who finished their post-graduate studies would be so gifted that they would remain at the institute as a member of the basic staff of creative scientific workers. Thus the institute would increase.

Such a method of taking care of students from their university-days, a thorough and continuous check of their abilities, represent from my standpoint for the present the only right way of selecting young scientific manpower. For this purpose one should spare no efforts, not only because young scientists are our future but also because they are our present. As a scientist grows old only young men, his disciples, can keep his mind lively. Any disciple working in his own field surely knows more about it than his teacher. And who is to teach a teacher, if not his disciples? The teacher owing to his experience defines the trend of work, but in the end the teacher is taught by his disciples, they make his knowledge more profound and his mental outlack broader. Without his disciples the scientist as a rule very soon begins to lack his creative ability and no longer makes progress. I never forgot the words of my great teacher Rutherford: "Kapitza", he used to say, "you know that it is only owing to my disciples that I am also feeling young". And now, when I myself am growing old, I feel that the contact with young men is the *modus vivendi* which preserves one from wasting away and maintains one's spirits and interest in everything new and progressive in science. Conservatism in science is for the scientist worse than a premature death—it is a hindrance to the development of science.

Let us pass now on to one more important aspect of the inter-relation of scientific work with the outside world, which seems to me to be unjustly ignored not only at scientific institutes but also in the Academy of Science as a whole. This is the problem of propagandizing science.

In our country, it is the popularization of science for a wide circle of people that is much discussed, but it has not entered one's mind that beside the popularization of science there is also the propaganda of science. Every great scientific achievement, every progress in science should not necessarily be popularized—and the scientist surely is not necessarily bound to popularize science—but it is up to him to propagandize it, i.e. to show his colleagues scientists the meaning of the achievement, to explain the role it is expected to play in science, to point out the effect that it may have on the development of the scientific thought, on our philosophical conceptions, on our technology, and so on. The propaganda of science is not a retelling of scientific thoughts in a simple language, but a creative process, because it is not at all easy to imagine and explain to others the effect that one or another scientific achievement may have on the development of science, technology, and culture on the whole. However, we are engaged little in the propaganda of science in this sense, and the propaganda is not given a sufficiently important place in the work of our scientists. Unfortunately, also at our institute due attention has not always been paid to this. The propaganda has been carried out here in the form of particular lectures delivered at scientific institutions, invitations of collaborators from other institutes to our scientific conferences, discussions in common on problems concerning the fields of science bordering upon ours, and so on.

Such a form of inter-relation between science and life is in our country carried out at random and in a way lacking organization. A consequence of this is a delay in the effect of one field of science on other ones and in inculcation of scientific achievements upon all aspects of the life of the country. We have to train the future propagandists of science and to organize their work. I always seek to stimulate a comprehensive discussion of any scientific work and not only have never suppressed scientific argumentations when they arose at scientific meetings but, on the contrary, found it useful to egg on an intense debate. Any comprehensive discussion of scientific studies should be welcomed. The more debates, the more revealed contradictions, and the more they are acute, the more incentives to a sound development of scientific ideas. Following this tendency, it seems that our institute has given reports at meetings of the Physical–Mathematical Division of the Academy of Sciences more frequently than other institutes.

I will approach now one of the most important forms of the influence of scientific work on culture—its effect on the development of up-to-date technology and industry.

What forms of organization should the influence of science on our technology and economy assume in our socialist country? This problem has frequently been discussed and is very acute in our country. I must say frankly that I cannot share a number of standpoints on this problem which are often suggested even by responsible chiefs with whom I had opportunity to talk. It appears to me that the conception of inter-relation between science and technology is often vulgarized in our country: a lot of people assume that technology should benefit immediately by every scientific work. These men estimate the work of any scientific institute only on the basis of the amplitude of concrete benefit reaped from its scientific achievement by one or another branch of industry. This is surely improper. Such an approach is naive and leads to a harmful simplification. Even a superficial study of the history of science and culture shows that any fundamental research affects invariably not only technology but also the over-all tenor of human life. It is quite clear that only owing to the fundamental studies and discoveries of Faraday completely new forms of the tools of human culture such as dynamos, the telephone, and so on, could be devised. But it is obvious that one cannot expect Faradays to make by themselves also the telephone and dynamos. Faraday was not an engineer and, moreover, the industry of his time was unable to put all of his ideas into practice. Bell, Siemens, Edison and other great engineers performed this several decades later. There are many such examples. But the fact that Faraday did not put into practice his ideas does not lower the validity of his great discoveries of the laws and properties of electric currents. In our country one often estimates scientific achievements only according to their practical consequences, and thus it turns out that the one who picks the apple is considered as the one who performed the main work, whereas in fact the one who planted the apple-tree was the one who produced the apple.

This standpoint, which I am calling in question, underestimates the importance of fundamental studies and, in particular, of the major part of research carried out by the scientists of the Academy of Sciences.

The problem of inter-relation between science and technology is rather many-sided. As an ordinary engineer calculates the braking of a truck or the stability of a construction he makes use of mechanical laws given by Newton. As an expert in patents rejects a "promising" proposal of a perpetuum mobile he starts from the energy-conservation law discovered by Meyer, and so on. As an engineer asks a scientist for advice, or for an explanation of an unelucidated phenomenon in a process of production, or how one or another mechanism can be calculated, and so forth—this is also an important aspect of inter-relation between science and technology. All of this occurs every day under various circumstances in tens or hundreds of instances. But this is so common that we do not speak about it, do not see it, and appreciate it rather little. However, this form of inter-relation is one important means of affecting engineering and industry by science. But in order that this effect may take place it is necessary that we should carry out fundamental research and have scientists able to lead it.

For example, our war engineering is as adequate as or in many respects even more so than that of our enemies. To what is this due? Of course, primarily to the existence in our country of fundamental research and scientists affecting in a number of implicit ways our engineering.

For instance, to what do we owe the high standard of our metallurgy? Surely, in the first place to the studies of Chernov and his disciples, and to the tradition in scientific approach in metallurgy, which they set up in the course of many years. Of course, a great merit is to be ascribed to engineers; they were able to derive all that was necessary from fundamental studies led by the founders of our scientific metallurgy. But without Chernov, Kurnakov and their followers our metallurgy certainly could not yield steel of such a good quality necessary for weapons with which our army is armed, or such an excellent armour as the one we are producing at present and without which engineers would be unable to construct first-class tanks.

Take one more example—our aircraft. To what do we owe its progress? Without the studies of Zhukovskii, Chaplygin and their school it certainly could not make progress. However, Chaplygin not only was unable to construct an airplane but also to draw its profile. He was a great mathematician like his great teacher Zhukovskii who laid down the foundations of aerodynamics of flight. Zhukovskii is worshiped throughout the world for his discovery of the basic theorem which underlies the calculation of the profile of airplane wings and owing to which the mechanism of the lifting power of the wing is elucidated. But was Zhukovskii to be asked also to construct the airplanes? His theorem is the fruitful apple-tree planted by him, and designers of airplanes will pick apples from it during many more centuries.

Of course, this influence of fundamental studies on technology should be organized better than at present, and should proceed through the propaganda about which I have talked. One has also to organize better the consultation of scientists engaged in industry. It is necessary that scientists should take more interest in those fields of technology in which their competence may have the greatest effect. If one may speak about a planning of science, then it should consist in stimulating the development of those fields of science which at a given moment may affect more efficiently the development of technology. But one cannot ask a great scientist to exert necessarily an influence on technology by deriving directly a practical consequence from his ideas.

I pass from these general introductory remarks on to a concrete picture of the inter-relation of our institute with technology. At first sight it may seem that what I am going to say contradicts the ideas that I have developed. However, this contradiction is due to the circumstance that I happen to take an interest, in addition to scientific studies, also in the problems of engineering. But this is, of course, an accidental circumstance which cannot be considered as a rule. It appears to me that it would be simplest to recount to you how the institute has developed its work on oxygen in industry.

In about 1930 our technological journals discussed with animation the very important problem on a wide application of oxygen in industry and on its possible effect on contemporary technology. A number of interesting articles and calculations by our advanced engineers pointed out how efficiently a low-cost oxygen supply may affect industry. Of these the intensification of the ferrous metal industry—blast-furnace melting, production of steel by oxygen blowing—was particularly challenging. Furthermore, the articles dealt with the problems of underground gasification, intensification of a number of chemical works, and so on. All these tempting and interesting prospects rested on the problem of a low-cost large-scale oxygen production, and so at the same time methods for this were proposed and discussed. I took an interest in this subject, drawing attention to several articles containing evident errors, and came to know various possibilities for the lowest-cost oxygen production. On the basis of contemporary physical concepts it could be shown that oxygen could be obtained at the lowest cost from air, where it is in a free state. Further, it could be shown—and I reported on this at the Academy of Sciences—that the lowest-cost way of producing oxygen at the contemporary standard of technology lies in liquefying air and in its subsequent distillation. Oxygen and nitrogen can be distilled from liquid air in the same way as alcohol from a mixture of alcohol and water. On the basis of general scientific considerations it could also be shown that in contemporary devices for the production of liquid air the efficiency was not larger than 10–15 per cent, and that the existing cycles of liquefaction and distillation are rather complicated. Furthermore, it could be shown that the most proper way of cutting the cost and simplifying these processes of oxygen production in large amounts would be to give up piston cooling machines and to pass on to turbo-rotational ones. It is interesting to note that the idea of constructing a cooling turbine, although put forward as far back as in 1890 by Rayleigh, has not as yet been successfully put in practice in spite of a number of attempts. It could be shown theoretically what was most likely the cause of the failure of these attempts, and how the errors were to be avoided. All these theoretical studies were interesting to carry out and this was, of course, the work of the scientist.

Having obtained these results, I presented them to engineers and specialists, and pointed to the way which in my opinion should be kept in order to set up a new technique for a low-cost oxygen production. They told me frankly that they found all of this too unrealistic and far from their contemporary ideas. In other words, our concept of technology was inadequate to adopt these new ideas.

As a matter of fact, I as a scientist could stop at this, publish my results and wait till our concept of technology grows adequate to adopt them and put them into practice. I know now that by this theoretical study I outlined the whole of the work that I carried out as an engineer for the past four years and which, as I initially assumed, was to be performed by our industry. I might rightfully stop at this theoretical work if I were not an engineer and if, speaking frankly, I were not seized by the fervour of an engineer. I was told that the ideas which I put forward as a scientist were unrealistic. I made up my mind to take a step further.

Within $1\frac{1}{2}$–2 years I constructed at the institute a machine for liquid-air production on these new principles. The aforesaid general theoretical assumptions proved to be justified. The machine underwent an examination by a governmental commission of experts. A decree of the Economical Council charged a plant to take over our scientific and technological experiment and to develop further our work. I thought that at this stage I could be reassured—the plant would work out new devices and develop them in the same sense. I assumed that from our laboratory model, which gave all necessary characteristics and thus proved all the basic theoretical assumptions, industry would develop a new technique for a low-cost oxygen production. But it turned out different. Althrough the plant was given fairly strict instructions by the government, it did not fulfill them.

Looking closely at the situation at the plant one could easily understand the cause of the delay in the development and inculcation of new devices. At the given plant there were gifted young engineers and constructors who got very interested in our undertaking. Some of these are also at present working with me. The general attitude of the plant staff toward the new task could not be called hostile. It admitted the usefulness and interest of the novelty, but the workers at the plant had simply no time for it. They had to do their everyday work and, above all, to fulfil the basic plan of the plant. Our arrangement certainly took much energy, interfered with the fulfilment of the plan and, as a small production, was of no importance in the annual balance of the plant. I think that the plant attitude toward new creative undertakings can best be characterized by recalling and somewhat paraphrasing some lines from *Faust*. Perhaps you remember them; applied to the given case they may read as follows:*

> Upon the highest concepts of the mind
> There grows an alien and more alien mould;
> When we but fill our annual plan completely,
> That which is better is labelled a fraud, a blind.

Our plants would like to have a conscientious attitude toward new scientific achievements, but life imposes upon them conditions such that the fulfilment of the plan appears to them to be of the primary importance. After a year of the work it became clear that no hopes could be cherished for the plant to develop on its own the problem of low-cost oxygen production.

It was then decided that our tactics were to be changed. The task was given to another plant, where was set up a special department and construction bureau engaged exclusively in our undertaking. By a decree of the Economical Council the institute was charged with the selection of the staff of this department and with the guidance of the work.

Meanwhile, to avoid a waste of time, the institute carried out the work which, as we assumed, industry was expected to do. From the device for the liquid-air production we passed on to working out new cycles, i.e. to the construction of a device for the liquid-oxygen production. We continued to check our theoretical designs and obtained liquid oxygen by turbo-engines. We also wanted to find out for how many hours in succession our devices were able to work continuously, and to what operating conditions at the plant they were to stand up for themselves. Therefore, although the oxygen device at our institute operated correctly, it could nevertheless not be foreseen whether it already grew up to an industry model.

The work at the new plant was developing better than at the preceding one but nevertheless slowly and, although we guided the department, our interference, being that of an outsider, had not always proceeded smoothly. In 1–1½ years one succeeded in constructing several devices and to pass them to industry. It is difficult to say how the undertaking would further develop, because the war started, and the new form of interrelation with industry ended.

The experience from the work with the plants taught us a lot. It showed that in the industry there were creative engineers and an aspiration for new techniques. From the very first step of our work on oxygen we met with a great help, support and interest in all our undertakings on the part of the government. It is only owing to this that our work made progress. What in this case hampered the work? Undoubtedly—organization! Our industrial organization is inadequate for a rapid and smooth mastering of new ideas of technology. But I have no doubts that in our economic system it is possible to find out and set up those organizational forms which would make it possible to inculcate and develop smoothly and rapidly new ideas of technology and to affect industry largely by science. However, these forms have as yet not been found, and are to be sought for.

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* Paraphrase of the translation by L. MacNeice, Faber & Faber, 1951.

The war intensifies the demand of the country for oxygen. One has to do everything in one's power to work out the machines to an industrial type and study the problems of endurance of the exploitation. We did this at Kazan where our institute was evacuated. At the same time, on the basis of the Kazan experience and according to the designs, large industrial units, which are beginning to operate, have been urgently built under the guidance of and jointly with the institute.

The war and forthcoming national-economic tasks with which our country will have to cope in the post-war period make the oxygen problem very acute. We had to do our best to derive benefit for our country from all the possibilities which our method of producing oxygen yields to industry. I cannot go into details of the measures taken, and will mention only that there is now built an independent central board—an industrial department for oxygen—one of the main tasks of which is to work out and inculcate the devices of our type. The central board has its own plant. I am charged to lead this organization.

The theme of my report does not allow me to consider in more detail the tasks put before the Central Board for Oxygen and the simultaneously established Technical Council for Inculcation and Use of Oxygen. I will only mention that the idea underlying this organization represents an attempt to create an organization connecting fundamental research with industry and, through use of oxygen, to intensify our metallurgy, chemical industry, power engineering and so on.

Here it seems that a contradiction appears with my basic theses, but it can readily be removed if one assumes that there are two Kapitzas—one a scientist and the other one an engineer. During the war the scientist had to give up his place to the engineer. As an engineer, I bent every effort to set up an industrial organization such that it may be able to assimilate and inculcate new scientific ideas. It is difficult to foresee what will come of this attempt, but in any case the war circumstances require that every effort should be made in order to achieve success.

All of this certainly does not contradict what I said in the beginning. All of this happened by the single chance of my being able to work both as a scientist and an engineer. You know there are cases that a man has two professions. For instance, Borodin was both a chemist and a composer. But this cannot be made a rule and held up as an example. When you are listening to a singer you do not ask him to sing necessarily to his own accompaniment. Therefore one also cannot ask a scientist that he should necessarily find the output and bring his scientific studies to industrial results. Certain scientists have the required bent for engineering and, of course, this chance should then be used. But if a scientist has no such gift, then it can be only harmful to incite him to do something of which he is not capable. I shall quote the example of academician N. N. Semyonov. The studies of N. N. Semyonov on chain reactions and burning are among the most outstanding scientific studies performed in the Soviet Union. The theory of burning, theory of explosions and theory of detonations, given by his studies and the studies of his school, have a colossal and universally recognized effect on the contemporary development of internal-combustion engines, explosives and a number of other branches of technology. In our country as well as abroad, wherever one has to study the process of burning, the name of Semyonov is cited as the most important one. But if Semyonov would try to construct by himself an internal-combustion engine or to lead such a construction, not much would come of it, while his time and forces would be drawn away from fundamental studies where he proved to be a great master. We esteem N. N. Semyonov as a great Russian scientist, as the pride of our theoretical thought, and his studies in theoretical chemistry will surely be appreciated by many generations. But as an engineer he is below the average. And if a singer is not apt to accompany his songs, why stimulate him to do this? Is it not better to train separately accompanists? However, it should be admitted that in our industry one usually does not bother about the training of appropriate manpower for bringing into practice up-to-date technology. Speaking frankly, this is our serious disadvantage which should be combated. But it would be improper to charge our great scientists with this task.

It appears to me that this sore subject—the inter-relation between science and industry—should be widely discussed in order to find out the sound forms of this inter-relation which is so necessary for our rapid cultural development. One should avoid vulgarizations in the statement of the problem, such as, for example, the following unfounded demand: all our scientists should necessarily inculcate by themselves the results of their studies, as it has frequently been done even by the Presidium of the Academy of Sciences. I will always protest against this. Science—fundamental research—has always furthered technology. The Soviet Union provides ample opportunity for this effect to become the most efficient. But these problems cannot be reduced to a primitiveness.

Allow me now to touch upon the last problem—the organizational structure of the institute. Those tasks which the institute put before itself and which I depicted to you undoubtedly affect its structure. At our institute there is a not very numerous permanent staff of scientific workers, and also a staff of temporary working scientists and post-graduates. Only one third of the manpower refers to the permanent staff, whereas the temporary staff amounted to two thirds of the manpower. This had necessarily a certain effect on the entire structure of the institute. Since the temporary staff is not paid by the institute, it is natural that the rate of our service personnel does not correspond to adopted norms referring merely to the permanent scientific staff. From the bookkeeping standpoint this was frequently noted as an overexpenditure, but if the number of the service personnel members is referred to all scientific workers at the institute then there is no longer a disparity. Moreover, it should be taken into account that the presence of temporary scientific workers at the institute makes it necessary to have a better qualified service personnel. If postgraduates are not looked after, they in the beginning inevitably mar facilities before they learn to work. A harmful effect on the scientific facilities of the institute may be exerted also by newly-arrived scientific workers, if they are not assisted by laboratory assistants. These last may also speed up the work, because they can help in setting up special devices adapted for low-temperature experiments, adjust the rather complicated thermometry of very low temperatures, show how liquid helium should be handled, and so on. In addition to the laboratory-assistant staff our institute has highly qualified technicians for a rapid manufacturing of special devices. It should be noted that nothing hampers, damps and dispirits scientific work so much as a sluggish manufacturing of devices for experiments. Therefore the good workshop at our institute is of much use for us.

Of great importance in the organization of the institute is the financial system. The financial system adopted at our scientific institutes is nearly the same as that at all other economic institutions. This is certainly improper. The aims and purposes of economic institutions and the organization of their work differ substantially from those of scientific institutes. Therefore the lack of correspondence between the financial system and the aims and purposes of the institute is a burden on the directors of our scientific institutes. But why has not even the Academy of Sciences made any serious attempt to change it? Instead of adjusting the system, the Academy tries to adapt itself to the system by a number of tricks and gadgets. We succeeded in obtaining for our institute an authorization to change and simplify considerably the finance. We started from the assumption that our institute should be very resourceful in the matter of organization. In fact, in the course of creative work it is difficult to foresee not only by a year but also by a month in advance how one or another piece of research will develop and what organizational forms and expenditures will be necessary in order that the work may advance most successfully. The versatility of our organization is due to the fact that we are allowed (and I find this very essential) not to register our staff in advance. The staff is set by the director of the institute according to the need. It would take us much time to dwell in detail on our financial system, but the point is that the institute may use its annual quota more freely than ordinary State budget institutions.

In order to persuade the Ministry of Finance that such a system is necessary we had to insist. Just at that time the Ministry of Finance had a tendency to introduce the so-called subject account: it found it ideal to account expenditures in detail, for each scientific subject in particular. In my discussions with the officers of the Ministry of Finance, I wrote the following "Would it be possible that in looking at Rembrandt's pictures you would like to know how much Rembrandt paid for his brushes and canvas? Then why in considering a scientific piece of work do you want to know how much was spent for equipment and materials?" When a scientific work gave remarkable results, then their value is completely out of proportion with the necessary financial expenses. The pecuniary value of a scientific work is always incommensurable with its cultural value. I asked: how much would the Ministry of Finance find admissible for Newton to spend for his work which led to the discovery of universal gravity?

But the Ministry of Finance restlessly objected. The discussion lasted for more than a half a year, and I think I would have not won if it were not for the

Soviet of National Commissars of the U.S.S.R. In the end, a simplified financial system which has saved the director a number of everyday troubles was devised for the institute. This led, for instance, to the fact that only one bookkeeper works at the institute, and that he has time to help, for example, in checking the equipment, taking notes and carrying out measurements. All of this facilitates and simplifies the life at the institute and sanitizes its spirit.

I shall now touch upon the last problem of the reorganization of work at the institute during the war.

The institute had not to reorientate itself considerably. The oxygen problem proved to be timely also in the war-time. The war induced all of us to seek to put into practice as fast as possible all the experience and knowledge accumulated in this field. We tried to organize our work in such a way that our experience on oxygen might soon be passed on to industry so that it might be as completely as possible used for the struggle against our enemies. It turned out that the studies at the institute in some other fields were also of urgent importance for war purposes. Unfortunately, for a number of reasons I cannot speak in more detail about this. Directing in this sense the over-all efforts of its workers, the institute had to reduce substantially the work on the themes about which I have spoken in the beginning of my report. We have concentrated our efforts almost entirely on the main trend, on the oxygen problem, in order to achieve by this definite and rapid results. We started from the fact that a scientific piece of work which could not be entirely completed and yield results during the war may turn out to be even harmful, if it draws away the efforts from a work that is more urgent.

At the close of my report, I wish to note that I tried to touch upon only the most general and basic problems of the organization of scientific work. Some of these are still far from being solved definitely by us. Unfortunately, the problem of organization of science is as yet little discussed in our country. Therefore I admit that a number of our solutions may still be considerably improved. But it appears to me that in the conditions of our country there are certainly many more unexplored possibilities. Even at this still imperfect organization of science which we have at present our fundamental research has already a more considerable effect on technology and on the whole of our life than we usually imagine. This effect comes to be through growing traditions created by fundamental research and through its connection by a number of invisible ties with our life and industry. It should be noted that without great scientific traditions, which began to be created by our scientists as far back as in the time of Lomonosov, we would not have good guns, strong armours and fast airplanes, although none of our academicians is able to construct airplanes or let off a gun.

We are still unaware of the possibilities at the disposal of our country, of the forces inspired by the close relationship of our science with life, of the opportunities offered by the Soviet Government for scientific work. We have still not taken advantage of the great freedom which exists in our country for the development of scientific thought. There are still many mistakes, the great role which the Academy of Sciences is called upon to play is sometimes not understood, and even the Academy of Sciences itself sometimes belittles its importance. But nevertheless our achievements are quite evident.

My colleague scientists! We are called upon to contribute to a great endeavour in a great country, and it is we that should first esteem this endeavour and take care of its development.

17. PROBLEMS OF INTENSIFICATION OF TECHNOLOGICAL PROCESSES BY OXYGEN

THE intensification of production processes appears to be one of the main tasks of contemporary technology; the progress of the latter leads to it. Pedestrian traffic has been superseded by the use of horses, horses have been replaced by cars, and the velocity of motion increased even more with the appearance of airplanes. Thus the intensification of the process of motion proceeds. The development of technology in most of its branches leads to the necessity of intensifying basic technological processes by raising the concentration of oxygen taking part in them.

Processes in which oxygen is being consumed are rather numerous. Man uses oxygen in the process of respiration. When he is sick, this process must be intensified, and so an additional amount of oxygen is then to be supplied to the organism. But the use of oxygen for medical purposes is not very large, and does not constitute any technical problem. The operation of any internal-combustion engine and the furnace of any boiler cannot do without oxygen. The whole of metallurgy is based on the use of oxygen. We shall not make a mistake if we state that not less than 80 per cent of all processes taking place in technology are based on oxygen. Wishing to intensify them, we have to increase the amount of oxygen supplied together with air and, in certain extreme cases, for example, in autogenous welding, pure oxygen separated from nitrogen must be introduced into the process. For the time being it is important to find out in which processes it pays to introduce oxygen, and in which ones it does not.

The solution will depend on the processes themselves and on the method of producing oxygen. First of all, one has to select processes in which it pays to apply oxygen, even if it is relatively expensive. Thereupon one has to examine existing methods of producing oxygen.

In which branches of technology does the application of oxygen promise to be most advantageous and economical?

Analysing the relative advantageousness of applying oxygen, it can be stated with a fair certainty that the application of oxygen is most efficient in processes associated with high temperatures (1000–1500°C or higher). In these processes the application of oxygen or oxygen-enriched air has a much stronger economical effect than in those in which lower temperatures are necessary. We shall illustrate this by an example.

П. Л. Капица, Проблемы интенсификации технологических процессов кислородом, *Кислород*, 1, 1 (1944).

From a report presented at a meeting of the Technical Council for the Use of Oxygen, held on 10th August 1943.

One would think that it would pay to blow oxygen into any boiler furnace in which coal is burnt. By this the following advantages would be gained: the carying away of heat in the smoke-stack by outgoing gases would be reduced, because it would not be necessary to let out nitrogen from the air, which is useless for combustion but gets heated in the furnace; the size of blast would be reduced, the possibility would be opened up of a more economical combustion of fluid, powdered and low-caloricity fuels, and so on. However, calculations show that, at the lowest cost of oxygen technically attainable at present, its application is still unprofitable in terms of money, because in boiler furnaces we have to do with not very high temperatures—not above 500–600°C. But, if we consider metallurgy, in particular the blast-furnace, an appreciably better effect can be expected. Thus, the application of oxygen in ferrous metal industry is of the greatest interest. This is determined also by the importance of metallurgy, which represents the basis of the whole of industry. The industrial potential of a country is determined by the number of tons of cast iron and steel smelted per man, which is one of the best indices of the status and standard of technology in the country. It is known that in America about a hundred million tons of steel are smelted per year, and it is undoubtedly owing to this that the American industry can be considered as the most developed.

Even the most general calculations concerning the application of oxygen in metallurgy yield very significant figures.

In the process of blast-furnace melting, especially in producing ferro-alloys, very high temperatures are developed, reaching 1800°C. Under these conditions the application of oxygen proves to be extremely advantageous, confirming the rule that its profitableness increases with increasing temperature of the process. Academician I. P. Bardin calculated that, applying oxygen in metallurgical works, one can considerably reduce the labour force, the cost of cast iron and capital investment.

With regard to steel production the situation is about the same. The possible economy in the cost of the final product by introducing oxygen blast will be here even more considerable.

In the production of cast iron and steel the situation is more or less satisfactory: there is already a certain amount of experimental material on the application of oxygen. Several blast-furnaces, for instance, have already worked with oxygen, and the economics of the process is more or less clear.

But there are other branches of metallurgy, where we can speak for the moment only on prospects, and where no experimental work has been done as yet. No experiments on the production of nickel with oxygen blast have been carried out, although this apparently also may be economical. The application of oxygen in pyrometallurgy of copper and other non-ferrous metals is very promising. The production of aluminium by the oxygen method instead of by the electrolytical one is particularly interesting. However, the feasibility of these processes is practically not yet proved.

In the ferrous metal industry we rest upon reliable empirical material. Abroad there are already blast-furnaces which work with oxygen-enriched blast. The XVIIIth Congress of the Communist Party of the Soviet Union raised the question about the transition of the ferrous metal industry to oxygen blast. In the non-ferrous metal industry it will be necessary first to perform experimental work and to undertake preliminary research. We shall have to organize, guide and follow them.

Among a wide range of processes in the non-ferrous metal industry, where oxygen can be applied very efficiently, there arises a very interesting problem about the intensification of gold output. If oxygen is supplied to baths in which gold is reduced, then its output increases by 10–15 per cent, while the process is speeded up 3–4 times. For this a rather small amount of oxygen is necessary. Whereas the ferrous metal industry demand for oxygen comes to ten thousand or so cubic metres per hour, here such an amount is demanded per year· But if we extract gold by 10–15 per cent more from the same amount of gold sand, then, taking account of the large output of this precious metal, the saving will amount to a pretty sum.

The application of oxygen in the chemical industry will have an essential effect. For the present there are, in the main, tentative calculations. An important branch of chemistry, in which the application of oxygen is possible, is the production of calcium carbide. In many other branches of chemistry, for example, in the production of nitric, sulphuric and phosphoric acid and so on, the study of the application of oxygen for the intensification of the processes also deserves attention.

The application of oxygen in fuel gasification is of great interest. When fuel is gasified by the usual methods (for example, in car gasogenerators) a gas of a caloricity of 1400 cal/m^3 is obtained. Such a gas is inconvenient to transport over large distances, while no substantially more valuable gas can be obtained in gasifying fuels with ordinary air. If a gas of a caloricity of 5000 cal/m^3 could be obtained, which could be achieved by means of oxygen blast, such a gas would be cheaper to transport over large distances. Thus, for instance, having under the ground of Moscow a number of different kinds of cheap fuels (brown coal, peat), according to existing calculations it seems very tempting to transport this fuel not by trains but through pipes in the form of high-caloricity combustible gases. Such a transition to gas of the industry of Moscow or any other town seems to be a challenging task, if one takes into account the possibility of getting rid of smoke above the town, of relieving traffic of fuel transportations within a radius of 300–400 km, and so on. This is again a problem to be solved only by means of oxygen blast.

Gasification of coal *in situ* is also of great interest, and appears to be one of the most challenging problems in contemporary power engineering. However, the processes of gasification of coal in place are still insufficiently worked out. In any case, they cannot do without oxygen-enriched blast.

Very interesting problems on the production of explosives—oxiliquits—are associated with the application of oxygen. A mixture of coal or another oxidizable organic substance and liquid oxygen gives an excellent explosive which costs 20–40 times less than TNT, ammonal and so on, and is in fact based upon an infinite source of raw materials. Here sufficient has already been done. Oxiliquits as explosives have already found application in mines and pits as well as in military operations.

This is the list of some most important problems which are first of all to be worked on in introducing oxygen into various branches of industry. The tasks put before us are very important, and one can cope with them only by working with a great enthusiasm and according to a definite plan.

One must take into consideration one more great difficulty which exists in any pursuit. Let us see how any great problem develops in practice. It always develops in a spiral, otherwise it represents a vicious circle. In fact, if one asks a blast-furnace engineer why no oxygen blast is applied for smelting cast iron, he will say: "Because we do not dispose of a sufficient amount of oxygen". If one asks an oxygen engineer why the oxygen industry develops so slowly, he will answer: "There is no large-scale demand for oxygen in our country". Thus, the blast-furnace engineer does not construct any blast-furnaces with oxygen blast, since he is not sure that they will be provided with a sufficient amount of oxygen, while the oxygen engineer does not build any oxygen plants, being not sure that oxygen will meet a ready sale. In capitalistic countries such a vicious circle is resolved slowly and with difficulty, because there the question of capital investment is decisive. In our conditions the situation is quite different. A risk of ten million or so roubles is for our country not the same thing as for a capitalistic one. We can undertake experiments much more freely. Of course, the risk will be great and there will be many failures, but we have not to be afraid of them.

An additional difficulty consists in the fact that, whereas we know blast-furnace production well and can forecast possible changes to come in it, a large-scale oxygen production is quite a new thing for us.

The Institute of Physical Problems of the Academy of Sciences of the USSR has recently succeeded under my guidance in devising new methods of producing oxygen, based on the application of low pressures. This possibility presented itself owing to the fact that our oxygen production is based not on a piston method but on a turbine method. The economy of turbo-plants, as shown by the whole of previous technieal experiments on their utilization, is manifested most clearly in the case of large plants and large-scale production. Small Laval turbines proved to be non-economical, but the economy of ten and hundred kilowatt plants equiped with high-capacity turbines is incontestable.

The profitableness of turbines increases with their size. Furthermore, turbines are more durable, they are not used up so quickly as piston engines, and capital investments in applying turbines are considerably smaller (calculations show that capital investments in a turbo-plant will be three times smaller, with higher profitableness, than in piston units). As to oxygen plants, they cannot at all be built on a large scale on the basis of piston methods. We think it is only on the turbine principle that we shall be able to build oxygen plants on a scale necessary to yield oxygen, for example, for a large blast-furnace, which is the greatest consumer of oxygen.

The new methods of producing oxygen incited the Soviet Government to take a number of decisions on the realization of oxygen production. Thus there was formed the Central Board on Oxygen attached to the Council of People's

Commissars of the USSR, which is called upon to inculcate upon industry new methods of producing liquid and gaseous oxygen, as well as to provide all branches of industry with oxygen plants of the turbine type.

The most important organ of the Central Board on Oxygen is its Technical Council, which directs and gives advice on the inculcation upon industry of new methods of producing oxygen and on its application for the intensification of technological processes in a number of the branches of industry of the USSR. The Technical Council consists in part of the leading figures in different branches of the national economy of the USSR. The most important problems associated with the application of oxygen in industry are discussed at regular meetings of the Technical Council. A considerable part of rewritten shorthand records of reports and discussions from the meetings of the Technical Council will be published in periodicals.

18. ON THE SUPERFLUIDITY OF LIQUID HELIUM II

Two and a half years ago, I spoke to the Academy of Sciencies of the USSR about the superfluidity of liquid helium.* Since then, we have continued work in that field and obtained new results which, in our opinion, are of scientific interest and which make it possible to elucidate more fully these interesting phenomena. However, before speaking about these new phenomena, I must remind you, if only in general terms, of the contents of my earlier talk, which some of you may not have heard. Otherwise, I fear, our recent work, which is a continuation of the earlier experiments, will not be sufficiently understood.

The study of liquid helium and its properties is part of the physics of very low temperatures. It is one of the regions of physics where one attempts to study natural phenomena under extreme conditions. Academician A. A. Baikov, Vice-President of the Academy of Sciences of the USSR, has in his introductory talk to this meeting indicated—in my opinion completely accurately—that we may expect the largest advances—indeed, large-scale changes—in technique when we go to extreme conditions. This is true even more strongly of science. We can expect discoveries of interesting phenomena most likely when we study nature in the most extreme conditions admitted by her, such as in strong magnetic fields, high pressures, large electrical fields, and so on, and also in the region of extreme cold when we approach the absolute zero. We can also look forward to observing new phenomena: properties of nature which under normal conditions either escape observation, or simply do not occur. The temperature region near the absolute zero is, in that respect, particularly interesting. The investigations of the last decade have clearly confirmed this point of view.

Let me remind you of what is meant by the absolute zero of the temperature scale which is defined as −273·13°C. It is well known that it is impossible ever to reach the absolute zero. The usual, schoolboy, definition of the absolute zero is the temperature at which the thermal motion of matter ceases. The definition is, however, inaccurate. Modern ideas, based upon quantum theory, postulate the existence of motion at the absolute zero. The energy of this motion is completely determined and it corresponds to the minimum molecular motion which can exist in a given substance. Let me give a simple example. If you heat a substance greatly, the atomic electrons which move around the atomic nucleus in well-defined orbits will be torn away under the influence of temperature motion, they will fly off: so-called dissociation will

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П. Л. Капица, Сверхтекучесть жидкого гелия II, *Nauka i Zhizn'*, **3**, **13** (1944).

Text of a talk given on 27th September 1943 to the Academy of Sciences of the USSR.

* See Chapter 12 of this volume, p. 73.

set in. When we cool the substance down, the motion of the atoms slows down, the electrons begin to return to their orbits and they retain their motion right down to the absolute zero. Apart from the motion of the electrons along the orbits of each atom separately, there are, however, in a solid a whole range of combined motions which, according to modern ideas, will still be present at the absolute zero. Due to this so-called degenerate motion, there may appear in this temperature region completely new phenomena which we cannot observe at normal temperatures. One of these interesting phenomena, which by now is widely known, was more than thirty years ago discovered by Kamerlingh Onnes; this is superconductivity. This phenomenon consists in that at very low temperatures in some conductors an electrical current may flow without resistance and without heat production. Experiments show that if a current is produced by induction in a closed superconductor, it will flow without producing heat and without diminution for as long as the experimenter is able to observe it. Another phenomenon which can only be observed at very low temperatures is the *superfluidity* found by us five years ago in liquid helium.

Liquid helium itself is used as cooling agent when we study these and other phenomena near the absolute zero. Liquid helium is the only known substance which even at the lowest temperatures, right down to a few thousandths of a degree from the absolute zero, remains liquid under normal pressure and does not turn into a solid. It can be made solid, but only under a pressure of at least twenty-five atmospheres.

Liquid helium by itself is an extraordinarily interesting subject for study. Helium liquifies at a temperature of 4·2°K and forms a light—it weighs between seven and eight times less than water—and transparent liquid. Because the specific heat of liquid helium is low, it must, to ensure good thermal insulation, be kept during the experiment in a Dewar vessel, which itself is surrounded by another similar vessel filled with liquid air. There are appreciable technical difficulties connected with experiments involving liquid helium. This is clear from the fact that up to the present only in a few low-temperature laboratories in the whole world liquid helium is available in sufficient quantities.

If we lower the temperature of liquid helium from its boiling point (4·2°K), we find that when we reach a temperature of 2·19°K, helium undergoes a change, and one usually says that helium I becomes helium II. This temperature is called the λ-point. In its initial state, liquid helium boils continuously, thanks to the small influx of heat which it is difficult to avoid even with the best thermal insulation. Below the λ-point, helium suddenly ceases to boil and its surface becomes smooth; this is connected with a change in several physical properties of liquid helium. Kamerlingh-Onnes was the first to observe this new state of liquid helium and Keesom was the first to study it and to show its extraordinarily curious behaviour.

Keesom found that helium II in that state acquires a large thermal conductivity. Its thermal conductivity, studied in capillaries, turned out to be many times larger than, for instance, the thermal conductivity of copper or silver, metals which conduct heat best. Keesom called liquid helium II, for this reason, a super-heat-conducting substance. I have repeated Keesom's experiments under slightly altered conditions and found an even larger thermal conductivity.

An attempt to explain the experimental data on the basis of present-day ideas about thermal conductivity revealed large discrepancies between theory and experiment. I shall not go into a detailed description of the rather complicated ideas on thermal conductivity as they were given basically by Debye. We can give the following physical picture of thermal conductivity: when we increase the temperature of a body somewhere, the average speed of the vibrational motion of the molecules of the substance increases; immediately an equalising process will start: the "hotter", that is, the more excited molecules act upon their neighbours and set them in motion. This process of successive equalisation of velocities will be extended further and further from the heated spot, that is, a process of heat propagation takes place which we call thermal conduction. A more detailed analysis, based upon these ideas of thermal conductivity, shows that there exists for every body in nature a limiting quantity of heat which can flow through it per unit time. It turned out that the very large thermal conductivity which was observed experimentally in our latest experiments on liquid helium II cannot be explained by the foregoing ideas. A way-out of this contradiction can be looked for either by renouncing the basic ideas of the mechanism of heat conduction which have been firmly established in science, or we must admit that the phenomenon of thermal conductivity in helium II originates from some new mechanism.

It is well known that heat can be transferred not only through the mechanism described a moment ago which is the way it is propagated in solids and in the way it was assumed to be propagated in liquid helium in narrow capillaries. Heat can also be transferred in liquids and gases by so-called convection currents. Convection currents in air, for instance, are well known to each of you: you have often felt them when holding a hand over a hot radiator. The same hand would not feel any heat at all if you held it at the same distance from the same radiator, but underneath it, since underneath there are no hot air currents emerging; they carry the heat upwards by convection. If it is impossible to explain the intense heat transfer in liquid helium by the usual thermal conduction mechanism, it seemed to me that, perhaps, convection heat transfer is the mechanism which occurs here. We must, in that case, assume that it is extraordinarily easy to produce in liquid helium II a current of the liquid and that this causes the extraordinarily large ability of helium II to transfer heat. Calculations showed that the enormous intensity with which heat is transferred in liquid helium can only be realised through convection currents which are such that they can flow in the liquid with unusual ease. I, therefore, suggested, by analogy with superconductivity, that at very low temperatures helium II is a liquid with an extraordinary fluidity, that is, a liquid without viscosity. It then was necessary to verify this with experiments.

To observe a small viscosity and especially at low temperatures, turned out to be a difficult experimental problem. It was necessary to find a special method to measure it. Once we had found and developed the necessary method, the observations themselves did not take much time and it turned out that, indeed, the viscosity of liquid helium is vanishingly small. According to our latest measurements, it is not more than 10^{-11} poise. As the viscosity of ordinary water at room temperature is 0·01 poise, liquid helium turned out to be more than a thousand million times more fluid than water. It is difficult for such a fluid medium to exist, and, moreover, the limit given is not a limit for the viscosity, but for the sensitivity of our measurements. As yet we have no more sensitive method. I, therefore, suggested that there are all grounds for assuming that liquid helium has no viscosity; I called it a *superfluid*. Initially, this met with great objections. People looked in my experiments for experimental errors in the method, or in the measurements. The discovery of superfluidity in liquid helium was thus discussed from all sides and we can now, I think, assume that the existence of a superfluid state in helium II is generally accepted.

When this phenomenon was postulated first of all, it seemed to us that the superfluidity of helium II was completely sufficient to explain the large heat conduction, observed in liquid helium, by appealing to the picture of the existence of convection currents which I have outlined to you a moment ago. In actual fact, however, it turned out that the situation was much more interesting and complicated than we thought originally.

It is somewhat difficult to expound how our ideas on this problem developed further. The audience is large and varied, and we have as yet not found for these problems the solution to a number of discrepancies. Nevertheless, I shall attempt to tell you what inconsistencies we encountered, how our ideas changed, and how we gradually put together a picture which seemed to us not to be infected by some absurd fantasies and which did not force us to forego all connexions with real experiments.

If we base ourselves upon the point of view of our normal mechanical theories which completely exhaustively describe the behaviour of normal substances under normal conditions, it turns out that experiments show that superfluid helium cannot transfer heat as intensively as a measurement of convection currents requires. Once again, we are faced with the difficulty of finding a mechanism which might cause the necessary fast flow of helium by convection. In the usual mechanism, the heat transfer by convection is caused by the motion of the medium in such a way that the hotter liquid or gas becomes somewhat less dense and thus moves upwards, floating, as it were, into the denser medium, while the colder, denser medium moves downwards, it "sinks". Mixing occurs, and it is clear that the cause of this motion is: gravitation. Calculations show, however, that these forces are insufficient in helium II to cause such a large thermal conductivity as is observed experimentally. This meant that this phenomenon is again unexplained. We must, therefore, look for its explanation to another, new mechanism. We finally succeeded by a series of experiments to find a completely new mechanism for the motion of liquid helium II.

It turned out that under the influence of a temperature difference in liquid helium II very strong currents are set up which are somewhat reminiscent of convection currents. The liquid is set in motion by a temperature difference, but this motion has an extremely peculiar character which is peculiar to liquid helium II; it is unknown in any other liquid or under any other circumstances. We first of all attempted to elucidate the essence of this motion by getting acquainted with its peculiar properties. We shall see what it looks like experimentally. I shall not describe the technical details of the experiment, as I did that in my previous talk. We can see the basic points from the sketch of Fig. 1.

The bulb 1 is submersed in superfluid helium II. A heating spiral 2 is placed in the wide part of this bulb, and the bulb is open at one side, 3. When a current flows through the heater 2, near the opening 3 of the bulb we observe a continuous current of helium flowing out from it. This current can be observed and even measured by means of a light vane, if it is suspended near the opening. The current falls upon it and deflects it.

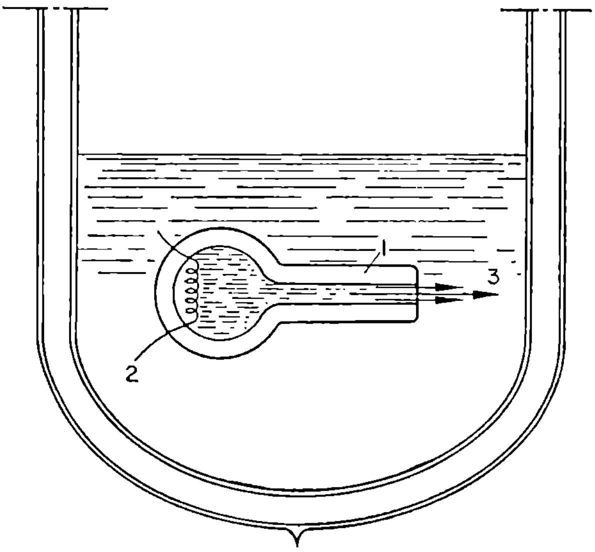

FIG. 1

A more effective and more instructive alteration of this experiment which is well-suited for a demonstration was photographed on a film. The set-up has been photographed while the device shown in Fig. 2 was in action. The glass "spider" consists of the "bulb" 2 on which are fastened several small pipes which are all bent in the same direction. In this way, the whole construction looks the same as the well-known "Segner wheel"—only externally, of course; looking at it we see immediately that the "spider" has no straight channel for the liquid to pass through. The bulb is put on the point of the sharp needle 1. The whole "spider" is immersed in liquid helium. The helium in the bulb can be heated by a beam of light passing through the lens 2. This beam of light, falling on the blackened part on the outside of the bulb, plays the role of the heater which in the previous experiment was played by a spiral. A continuous jet is emitted from the pipes—"legs"—of the spider, when the middle of the vessel is heated, just as in the previous experiment from the neck of the bulb. Under the pressure of the emerging jet, the whole "spider" starts to rotate, as can be seen on the screen.

It is difficult to photograph this experiment. Liquid helium is completely transparent and its diffraction coefficient for the light beam is such that it is very difficult to see it through the glass. Neither is it easy to perform an experiment under the conditions of very high illumination which are necessary for taking a film. The considerable skill of the camera-men of the Moscow newsreel company was, therefore, necessary to complete this filming.

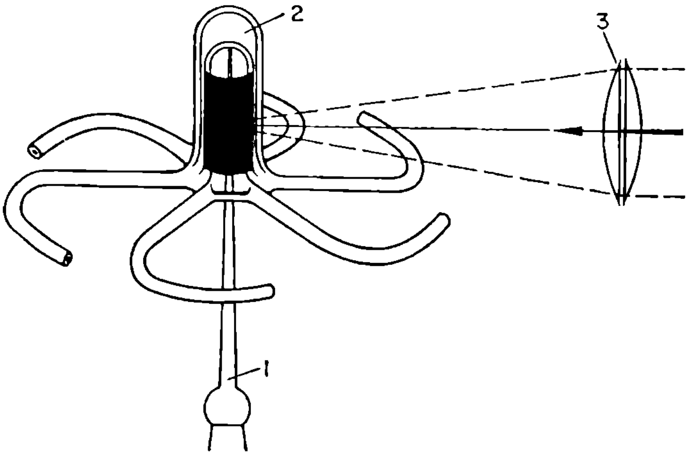

FIG. 2

Let us return to Fig. 1. I now draw your attention to the very paradoxical aspects of this experiment. If we observe all the time that liquid is flowing from the bulb and if the interior of the bulb does not become a vacuum, it means that liquid must also flow all the time into the bulb. How does the liquid get into the bulb? It cannot flow out of it, without getting into it. The walls of the bulb are double ones and the space between them is evacuated and it is clear that the liquid cannot pass through them. By means of a vane placed in very different positions near the mouth, we could not observe any reverse current at all. We decided, therefore, initially that the current must take place along a very thin layer at the wall itself—in that case, it could not be observed by the vane. It turned out, however, from further experiments that this hypothesis is insufficient. I began to change the experimental conditions: instead of a bulb with a wide opening, I used a very narrow gap. The idea of these experiments lay in the possibility of using the whole cross-section of the gap for the reverse current at the wall and thus attempting to change the character of the phenomena observed. The gap in these experiments was prepared very accurately from carefully, optically-polished surfaces and had a width of up to 0·14 μ, that is, a width of the order of a ten-thousandth of a millimeter. We did not, however, observe any change in the character of the phenomena.

The phenomena remained thus extremely puzzling.

Before I tell you how we can now explain it, I wish to remind you of a few other experiments.

First of all, permit me to dwell upon the idea of the reversibility of thermal phenomena. Carnot was the first to establish this concept more than a hundred years ago; it gives the extraordinarily important connexion between different possibilities for changing work into heat and the other way round. Reversible phenomena in thermodynamics are considered to be those theoretical processes where heat is changed into work, or, inversely, work into heat without any scattering of heat. There are no completely reversible processes at all in nature, but we can very closely approximate to them. The transition from heat to the motion of the helium which we, for example, observed in our "spider" (see Fig. 2 and Plate 17) must primarily be studied from this point of view. If the temperature difference between the helium in the bulb and the helium outside causes the motion of the helium and if this is a reversible phenomenon, there should theoretically exist also the reverse phenomenon: when a motion is induced in the helium, a temperature difference should arise. If these phenomena are reversible, they must be connected by well-defined quantitative relations.

In the experiments with gaps, we were able to show that when there is a drop in pressure which causes liquid helium to flow through the gap, a temperature difference appears, indeed. We were able to measure quantitatively all necessary quantities and to show that, indeed, all these phenomena in liquid helium II proceed reversibly in the thermodynamic sense. If we now bear in mind that helium II is superfluid and that there is thus no loss by friction when it flows, we see easily that the mechanism of temperature flow of helium works with a good coefficient of efficiency. In this way, for instance, our rotating "spider" of Fig. 2 and Plate 17 is an engine with a good coefficient of efficiency. Of course, there cannot be any practical application of such a mechanism, and it is difficult to expect that there will ever be one.

We must, however, note here that this remarkable thermodynamic property of helium II, which reveals a completely new method of changing heat reversibly directly into mechanical work, has no analogue whatever in the phenomena occurring in nature which were known to us up to now.

The reversibility of the thermomechanical—or rather thermodynamic—phenomena in liquid helium is for us an extremely important fact also for the further study of low temperature phenomena. Let us assume that we have a capillary 1 (Fig. 3) connecting two vessels at different levels. Between its ends we produce a pressure difference. We can do this by placing vessel 2 at the one end of the capillary higher than vessel 3 at the other end. Because of the peculiar properties of helium and the irreversibility of the process, we shall find a temperature difference occurring between the vessels 2 and 3 at the two ends of the capillary. The helium II in the lower vessel 3 becomes the colder one.

We have thus a method for lowering the temperature of helium II; it consists in forcing helium II to flow under pressure. Figure 3, of course, gives only a schematic picture of this principle; the experiment is, of course, in actual fact more complicated.

Since, however, this phenomenon remains reversible down to the lowest temperatures, the possibility arises to reach very interesting practical conclusions. If we force helium by pumping or by other means through thin capillary gaps into some space, the temperature in that space will be appreciably lowered. Repeating this operation several times, we obtain an *a priori* method of making the temperature arbitrarily low and we have thus found a way to approach the absolute zero arbitrarily closely. This conclusion is very important for an experimental physicist, since up to now, there does not as yet exist a method, not even a theoretical one, for approaching absolute zero arbitrarily closely. The most effective method so far has been the magnetic one—based upon the demagnetisation of paramagnetic salts connected with their cooling—which both theoretically and practically has its limitations. This method, indicated first by Langevin and further developed by Debye and Giauque, made it possible to reach temperatures of the order of one hundredth of a degree from the absolute zero. This magnetic method has, however, theoretical limits for obtaining low temperatures; these are caused by the interactions between the magnetic moments of the cooling salts. However, we cannot see any reasons why we cannot use these peculiar properties of liquid helium II as cooling agent and employ the absolute zero arbitrarily closely.

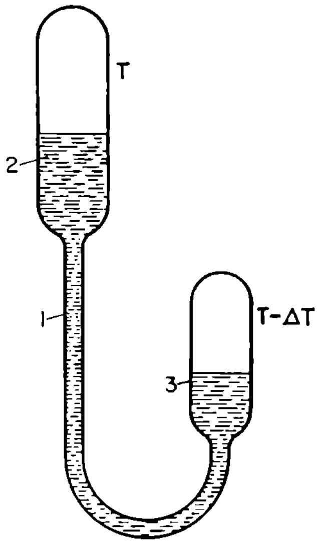

FIG. 3

Just before the war, we started to develop this method in our experiments and we had performed some successful experiments in that direction. I was able to obtain a lowering of the temperature by 0·4° by this method. After our evacuation to Kazan it was impossible to set up experiments with helium. We now propose to continue these experiments. Of course, obtaining temperatures very close to the absolute zero by a new method is a technically difficult problem and it is difficult to count at once on its success. There are many technical difficulties and success depends in many respects upon the skill and ingenuity of the experimentalist. All these possible difficulties do not mean, however, that there are any prohibitions of principle against approaching the absolute zero.

Let us now, however, turn to the theoretical explanation of the mechanism of the flow of liquid helium from the vessel when it is heated (Fig. 1). As I said before, I explained this phenomenon originally by the filling of the vessel by helium, through the flow of helium in the opposite direction in a thin layer. I assumed also that the energy state of helium II in that thin layer differed from the energy state of free helium II, and could thus explain the apparent large thermal conductivity of helium. I could also approximately evaluate the thickness of this layer by assuming that the flow velocity of the helium in it would not be excessively large. I also—as mentioned earlier—tried in these experiments to observe experimentally the thickness of this layer. To do that, I forced helium to flow in a very thin layer. I gradually reached a helium layer thickness of 0·00014 mm, but the experiment showed that the character of all phenomena remained unchanged. We needed thus to reconsider this explanation and this led to completely new ideas about the nature of hydrodynamic phenomena in helium II. The first sketch of these ideas is due to Tisza, but our scientific colleague L. Landau was the person who worked them out, gave them a theoretical foundation, and produced a hydrodynamic theory of the various phenomena.

I shall try to give you a very general picture of these ideas. The counterflow which I tried to explain by the flow of the helium in a single energy state along the wall with another energy state of the helium in the bulb, is, according to Landau's theory, replaced by a counterflow of the helium passing through itself. The explanation of this phenomenon as given by Landau seems as follows.

Liquid helium is, so to speak, a mixture of two liquids. These two components of liquid helium are in two different quantum states. Because of this, he could show that there may exist at the same time two opposite flows in the same liquid, such as we observed in the neck of the vessel of Fig. 1.

If this theoretical idea were not fully substantiated by experimental proofs, it would sound just like another idea which it would be very difficult to consider reasonable.

Landau's theory gives a good description of the main physical aspects of those two states in which helium can exist simultaneously at temperatures below the λ-point. As I said before, if helium after liquefaction is further cooled down, it remains an ordinary liquid down to the λ-point at 2·19°K. At that point, helium in a new state exists—as an impurity, so to speak—in the liquid according to Landau's theory. This new state is thermodynamically characterised by a zero entropy and physically by the absence of viscosity. This helium is liquid helium II in the state in which all of the helium finds itself at the absolute zero. At any other temperature, however, mixed in, so to speak, with this state will be helium in the normal state. When the temperature is lowered, the concentration of the helium in the normal states diminishes while, on the other hand, the superfluid state of helium begins to dominate. The whole of the helium must go into the superfluid state according to the theory only at the absolute zero. This picture is sufficient to explain the phenomena observed by us. The phenomenon observed in the experiment with the helium flowing from the bulb shown in Fig. 1 can, for instance, be explained as follows. Since helium in the superfluid state does not experience friction neither from the wall nor from the helium in the normal state, the current flowing along the capillary does not produce a friction reaction and can thus, so to speak, imperceptibly fill the vessel. On the other hand, helium in the normal state flows with friction from the vessel and this current is a normal liquid current, which has been studied for so long in hydrodynamics. This normal current is caught by the vane placed in front of the neck of the bulb in Fig. 1, while the current of the helium in the superfluid state meeting the vane cannot be observed by normal means.

We can also explain the large heat conductivity of helium II on the basis of this picture. It is clear that helium in a zero entropy state enters the vessel and helium in the normal state returns. To transform helium from the one state into the other state we must expend a considerable amount of heat. Such a process is a peculiar form of convection and gives the impression of a large thermal conductivity of helium II.

All these phenomena, the explanation of which requires the existence of complicated interactions between different states of the same liquid in the same volume, can only with difficulty be fitted into our usual framework even of physical thinking. Let me try to make it somewhat easier to get at least a superficial idea of this complicated picture of the thermal conductivity of helium II. I shall have recourse to the similarity with the coming and going of people, both with and without coats, along the passage in the cloakroom of a theatre. Those with a coat on will represent the normal helium atoms which in the neighbourhood of the heater—"in the cloakroom"—obtain the necessary energy, while those without a coat are the superfluid helium atoms. Unfortunately, the analogy is worse than incomplete, as the helium atoms in the state of zero entropy pass their brethren in the normal state without any interaction, while one after receiving one's overcoat cannot move through the crowd without a large friction.

We can on the basis of this picture explain why a temperature difference arises when helium II flows through narrow holes or gaps. As the helium in the superfluid state flows more easily, without friction, through small openings than helium in the normal state, a peculiar filtration occurs. After the helium has flown through, the concentration of the superfluid helium is increased; this corresponds to such a concentration of it that we must assume a lowering of the temperature.

There is not only qualitative, but also quantitative agreement between Landau's theory and the experiments. However, there are still several phenomena which are not covered by the theory. Their explanation lies in the future. On the other hand, the theory indicates some phenomena—such as the existence of two sound velocities—which we have not yet been able to observe. The theory does not yet evaluate the critical velocities which we have actually observed. It seems to me, however, that in the basic ideas the theory has very closely approached the essentials of an explanation of this amazing phenomenon and that it is an exceptionally valuable contribution to the study of this effect. Work on further interpretations of these phenomena is of great interest.

Now that we are back again in Moscow thanks to the great efforts of the Red Army and the heroic deeds of its soldiers, we can continue our scientific work on liquid helium which was interrupted two and a half years ago by the invasion of the German barbarians.

PLATE 18. Sir Isaac Newton.

19. ISAAC NEWTON

THE president of the Royal Society of London, Sir Henry Dale, charged Academician Vinogradov and me with representing the Society at the meeting of the Academy of Sciences, summoned for the celebration of the 300th anniversary of Isaac Newton, who was a member and chairman of the Royal Society. We have been charged with this honourable mission as Fellows of the Royal Society. If there were no war conditions, when the passage from England is made difficult, I am sure that the Royal Society would send here its delegates. But in view of the concurrence of circumstances allow me to greet the meeting on behalf of the Royal Society, and to say a few words on the occasion of this anniversary.

Of course, I cannot express what a compatriot of Newton would say in my place. Newton is for us a great scientist, to whom mankind will be grateful for ages for his contribution to science. But an Englishman can rightfully take pride in Newton as a son of his country, which educated him and afforded him opportunity to develop his work. We Soviet people take pride in our scientists of the past as well of the present, and have respect for this feeling in other nations.

The scientific activity of Newton was closely associated with the Royal Society almost since its very foundation. The seventeenth century was one of an astounding flourishing of natural sciences in England as well as in a number of other European countries. One who expressed the new trend in science and traced the ways of its developing was Francis Bacon. Assuming experiment as a basis of the development of natural science and delivering science from mysticism, he predicted that mankind will in such a way subjugate for its welfare forces of nature still unknown at that time. Being a brilliant publicist and a profound philosopher, Bacon carried with him a number of eminent men of England. Suffice it to say that *New Atlantis*, which appeared after his death, ran to ten editions during forty-three years—an extraordinary phenomenon for that time. It is hard to find in history another example of such a great influence of a philosopher on the trend of development of natural sciences. Bacon rightfully used to say that "a cripple going the right way may outrun a trotter, if the latter is running the wrong way".

In his *New Atlantis*, Bacon considered a systematic organization as an indispensable condition for the development of natural sciences, and, in spite of the

П. Л. Капица, Исаак Нютон, *Известия Академии Наук СССР, серия физическая*, 8, 169 (1944).

Adress on behalf on the Royal Society to a meeting held on 4th January 1943 in the large auditorium of the Moscow Palace of Learning; for the celebration of the 300th anniversary of Isaac Newton.

opinion settled at that time, he regarded physics as of paramount importance and as "the mother of all sciences". The ideas of Bacon were adopted by a group of scientists and progressive-minded people of England. This group started to meet regularly and to carry out and discuss experiments at their meetings. In the beginning these meetings were held in a tavern. The members of this society called themselves the "Invisible College". The first of these meetings was held in 1645. We can judge the composition of this society from such names as Robert Boyle and Robert Hooke, who are well known for their studies on the gaseous and solid state, further mathematician John Wallis, architect Christophor Wren. The last is maybe less well known outside England, where he achieved fame as the greatest architect who rebuilt London after the fire. Many of his most beautiful churches in London have now been demolished by bombs of the vandals of the twentieth century. It is interesting to note that Wren was Savilian professor of astronomy at Oxford University, but, in addition, he performed a number of important studies on applied mathematics, anatomy and zoology. He was one of the greatest erudites of his time. In spite of troubles due to the Civil War, the society was being progressively enlarged, and, when Charles II mounted the throne, he gave this society in 1662 a special charter determining its statute and rights. It is by this charter that the Royal Society of London was established for the development of natural sciences, having the king as the patron. The Society was granted an emblem with the motto *Nullius in verba.* This motto expresses the idea of Bacon that science should not rest upon authority but that every statement in science should be checked by fact.

Thus from a small group there arose the Society which has played an important role in the development of science in its country, where it is in charge of leading functions analogous to those of Academics of Sciences in other countries.

When the Royal Society began in 1663 its activity, it consisted only as to one third of scientists, while the majority of its members were persons who only were interested in science. Science was at that time very popular, and among the members of the Society we encounter the names of a number of statesmen, as, for example, Pepys, who was often present at its meetings and subsequently was even its chairman.

Beside Boyle, Hooke, Wallis and Wren, a Fellow of the Society since its foundation was also Isaac Barrow, a great mathematician and teacher of Newton. Barrow was educated at Trinity College in Cambridge, where before him Bacon studied and where Newton began and carried out his basic studies. Trinity College has maintained the high tradition in educating scientists to be: not long ago such scientists as Maxwell, J. J. Thomson and Rutherford came from its precincts.

When Newton came to Trinity College, Barrow was a professor and the first to hold the Lucasian chair. At those times a professor in England obtained his pay from a donation of a Maecenas—in the given case this was a certain Lucas. Barrow noted at once the extraordinary ability of his disciple Newton. When Newton was only twenty-seven years old Barrow decided to retire, in order to give his chair to Newton.

This fine gesture shows the outstanding human qualities of Barrow. It is interesting that this chair exists also now. It was held by a number of great scientists, the last but one of whom was Larmor, and is now held by Paul Dirac.

Under the influence of Barrow and the fellows of Trinity College surrounding him, Newton became a follower of Bacon's ideas and an adherent of the experimental trend in science. Already as a young man he carried out experiments: he made himself a sun-dial, a telescope and other devices, some of which are conserved in Trinity College and in the museum of the Royal Society. Among the relics there are also two rulers made from the apple-tree from the garden in Woolsthorpe, where Newton was born and where he lived. One may consider this tree to be just the famous one from which the fall of an apple made him discover universal gravitation. When he was only 28 years old, on the 11th January of 1671 he was elected a Fellow of the Royal Society, and from 30th November of 1703 he was its president, being re-elected as such till his death. Thus Newton was a Fellow of the Royal Society for 56 years, 24 of which he was its president. During these years Newton was in close contact with the Society, and frequently took part in discussions. In 1686 he submitted to the Society the manuscript of his famous work *Principia mathematica*, which by the order of the Society was published in a year.

Newton took active part in meetings and affairs of the Royal Society. It should be mentioned that the Royal Society, in contrast to all other Academies, has still the character of a non-state society. It is supported by fellowship dues, and only since 1852 has it obtained a subsidy from Parliament. In Newton's times the Society often experienced pecuniary embarassment, mainly due to the unpunctuality with which fellowship dues were paid in. Since the beginning of his presidency, Newton introduced a rule according to which the Fellows of the Society had to undertake a written, notarially asserted engagement as to a punctual payment of their fellowship dues, with the threat of a considerable penalty. Experience showed that this measure gave positive results. At critical moments Newton rescued the Society with his own means.

Since the very beginning of its existence the Royal Society has maintained contact with many foreign scientists. Sometimes foreigners were elected as Foreign Members of the Society, including our scientists Euler, Kruzenshtern, Struve, Chebyshev, Mechnikov, Pavlov, Timiryazev, Golitsyn and a number of Russian statesmen. One of the latter was Aleksander Men'shikov, the closest collaborator of Peter the First, who was elected as a Fellow of the Royal Society in 1714, even before the foundation of the Russian Academy of Sciences. Since Newton's times the Royal Society has maintained a lively contact with scientific societies the world over.

We—the scientists from freedom-loving and democratic countries—appreciate highly this unity of scientists the world over, for we know well that there is only one science serving for the common weal and providing mankind with means of subjugating nature and of increasing its well-being. This science resulted from the cooperation of the scientists of all countries, and each country did its bit as far as possible. Our meeting today shows how much we appreciate the work of all scientists. The connection between scientific societies and between scientists has been organizing our cooperative work during so many centuries.

And then suddenly, in the twentieth century, there appeared a senseless ideology of the existence of a superior race, which is ostensibly the only one able of leading the world and science. Madmen are pitiable, but if violent ones are in possession of arms they become socially dangerous, and they must be rendered harmless at any price, otherwise sound people will perish.

Here, where we have met to observe the anniversary of a great English scientist, the international spirit of science is particularly manifest, and the utmost danger to which the world culture is exposed is manifest as much. We should not forget for a moment this danger, and we have to recall that everything that Newton and other geniuses contributed to human thought will be lost, if we do not fight for equality, truth and freedom of human thought.

20. THE SCIENTIFIC ACTIVITY OF BENJAMIN FRANKLIN

Franklin was born in Boston, USA, in 1706, and died when he was 84 years old. His activity covered the whole of the eighteenth century and was closely associated with the rapid development of natural and social sciences which was wrought at that time. This was an epoch of "enlighteners", the epoch that preceded the period of radical social upheavals in Europe.

The name of Franklin went down in the history of the world culture not only as the name of a great scientist, one of the founders of the teaching on electricity, but also as the name of a great progressive-minded statesman and public figure of America who took an active part in the struggle for its liberation from the colonial conditions.

The contemporaries were unanimous in describing Franklin as an exceptionally fascinating man, widely educated, of humanitarian and broad outlook, an interesting and witty speaker. Franklin often travelled and spent many years abroad, mainly in England and France. There he had extensive contact with the progressive-minded people of his time, and toward the end of his life he became the most popular figure in Europe. In his country, America, Franklin is still one of the most esteemed persons of the entire history of USA. The versatile work and life of Franklin are well known, and many studies are devoted to these.

The fundamental scientific discoveries in the field of electricity were made by Franklin at the middle of the eighteenth century, before the work of Galvani and Volta, i.e., before the epoch of the galvanic current, and they refer to the initial period of mastering this powerful force of nature by science.

In the course of 200 years since the time of Franklin's research the teaching on electricity advanced so much that nowadays Franklin's work is studied in secondary schools only in classes where one starts to learn physics. All of us have been acquainted since our young days with the teaching on static electricity, although some of us might have forgotten what, in fact, Franklin did

П. Л. Капица, Научная деятельность Вениамина Франклина, *Успехи Физических Наук*, 58, 169 (1956).

П. Л. Капица, Научная деятельность В. Франклина, в *Жизнь для Науки*, Знание, Москва 1965, стр. 21.

Discourse given on the 17th January 1956 at a meeting of the Academy of Sciences of the USSR, the Soviet Committee for the Defence of Peace, the M. V. Lomonosov State University of Moscow, the Union of Writers of the USSR, and the Soviet Society for Cultural Contact with Foreign Countries, held in celebration of the 250th anniversary of Benjamin Franklin.

perform. For example, do all of us remember that the terms positive and negative poles were first introduced in science by Franklin?

A detailed description of the scientific work of Franklin would hardly arouse interest now, but it appears to me that the very history of the development of Franklin's research in the field of electricity is not only interesting but also useful for contemporary scientists. This follows from the fact the trail of the development of science, i.e., of the cognition of nature, followed by humanity is a single one. In our searching for scientific facts we are frequently led astray, which results in a waste of time. Therefore the less our deviation from the right way, the more rapid and economic the growth of our knowledge and of our mastering the forces of nature. In studying the history of science we find factors that favour a rapid development of science. From this standpoint the story of the development of Franklin's scientific work is of great interest.

Franklin performed his studies on electricity for a short time interval: only for seven years, from 1747 to 1753. He began to do scientific research when he was 41 years old. By that time Franklin already had become a man of substance. A typography, a journal, a famous almanac, and other printed matter produced by him in Philadelphia which was at that time still a provincial town were a great success. Franklin began to take interest in scientific work completely accidentally, after he had to attend a popular lecture with demonstrations on electricity. Such lectures enjoyed then a wide popularity, because a number of electric phenomena such as the repulsion and attraction of charged bodies, the electric spark, or the disagreeable feeling caused by a discharge through the human body were at that time new and completely out of the ordinary, and served as very good material for popular scientific lectures.

Not long before Franklin attended the lecture on electricity, there was discovered the Leyden jar which for the first time yielded a method of condensing an appreciable amount of electricity. The possibility to carry out experiments with a considerable amount of electricity in store made at once the demonstrations of electric phenomena more striking.

Franklin was very much interested in experiments on electricity, and in the course of seven years he devoted the major part of his time to scientific research. This work became then the leading one in the development of the teaching on electricity, and was generally recognized. In this short time interval Franklin was acknowledged as the leading scientist of his time.

Most great scientific societies or academies distinguished the scientific merits of Franklin by electing him as their member, while a number of universities conferred on him the honorary doctor's degree.

Naturally, the question arises how it could happen that Franklin, who was earlier never engaged in physics, was able to determine the trend of the development of an entire branch of science in only a few years of work, in a small American town far from the centres of the international science, and being an already mature man, standing by himself?

Moreover, this took place by the middle of the eighteenth century when science was led by highly competent scientists such as Newton, Huygens and

PLATE 19. Benjamin Franklin.

Plate 20. Benjamin Franklin.

Euler. Dilettantism is here out of the question. But how could Franklin achieve such results which could not be reached even by professional scientists?

It appears to me that the explanation is to be sought for in the fact that Franklin was the first to understand the essence of electric phenomena and, hence, to reveal the right trend of further investigations in this field. Many of us were witnesses of an analogous turning-point in the development of one of the most important fields of physics—radioactivity.

After the Becquerel discovery of the phenomenon of radioactivity in 1896, very abundant experimental material on physical phenomena associated with the radioactive property of matter was acculumated during several subsequent years. Various experimental data were lacking in coherence, since the essence of the very phenomenon of radioactivity was not understood. Rutherford was the first to find out that physical phenomena associated with radioactivity were at once accounted for if radioactivity was assumed to be a process of disintegration of matter. In order to see this it was necessary for Rutherford to be of great erudition, but the most important factors were his imaginativeness, perspicacity and audacity. At such initial stages in the development of science the exactness and punctuality inherent in professional scientists may rather hamper the advancement of such audacious assumptions.

In the initial stage in studying electricity it was necessary that such a challenging step should be made. And Franklin made it.

Before Franklin started his research there was already accumulated abundant experimental material, but the facts were incoherent; the hypothesis that he suggested not only united these facts into a consistent pattern but also pointed out the right trend of further investigations.

Franklin expounded his basic hypothesis in a letter sent to Peter Collinson in 1749. It gives a clear picture of the processes taking place when a body is being electricised. This picture has remained till now basically a correct one.

A passage from this letter reads: "Electricity consists of extremely small particles, since they can impregnate ordinary substances as compact as metals so easily and freely as if they experienced no considerable resistance".

Nowadays we call these "extremely small particles" the electrons. Further, Franklin considered every physical body as a sponge saturated with these particles of electricity. Electrification consists in that a body having an excess of electrical particles is charged positively, whereas if a body is deficient in these particles then it is charged negatively. Franklin proved this by a very obvious experiment.

Imagine two men standing on wax cushions, i.e. on insulators. One of these is electricizing a glass rod by rubbing it. Then, if he touches with it the other man, they both become electricized with respect to the earth, which is simply proved by the fact that any of these men produces a spark when touching an object connected with the earth. If immediately after getting electricized the two men standing on insulators touch each other, then this will give rise to a spark between them; thereupon their charge with respect to the earth will vanish. This is proved by the fact that now no spark will arise as they touch an object connected with the earth.

Franklin's hypothesis started from the material nature of electricity and explained simply these experiments. When the insulated man touched the other one with the glass rod, one of these men lost electric matter whereas the other one gained it in the same amount. One of these was charged positively and the other one negatively. When they touched each other then a discharge took place and, since the constant amount of electric matter was preserved, the previous equilibrium was established. Of course, Franklin had then no possibility to study experimentally the material character of electricity and hence he could not determine who in fact gained the electric matter and, consequently, was charged positively, and who lost it, i.e. was charged negatively. Therefore he assumed arbitrarily that the electricized glass was charged positively, perhaps supposing that the woollen stuff was rubbing the electricity into the glass. Only at the close of the last century, after the discovery of the particles of electricity—electrons—it became clear that it was not the positive electrode that accumulated the electric particles, as Franklin assumed, but the negative one. To retain the adopted denotations of the positive and negative polarity a negative charge was assigned to the electron.

I will present one more experiment of Franklin, which is also of great scientific interest.

The property of mutual repulsion of bodies whose charge is of the same sign was generalized by Franklin to charges on metal conductors. He assumed that the charges, being repelled from one another, would tend to get at the surface of the electricized metal body. He proved the validity of his assumption by the following experiment.

A metal tea-pot was put on an insulator and electricized. An experiment was to be devised which would prove that the charge was being distributed at the external surface of the tea-pot. To this end, inside the tea-pot there was put a chain which by means of an insulated holder could progressively be drawn out of the tea-pot. The degree of electrification of the tea-pot was determined from the repulsion of two balls suspended on filaments. The experiment consisted in raising the chain from the tea-pot by means of the insulated holder and in observing how the degree of electrification of the tea-pot decreased as the chain was being drawn out.

Franklin reasoned as follows. So long as the chain was inside the tea-pot its surface increased the inside surface of the tea-pot, whereas as the chain was being drawn out its surface increased the external surface of the tea-pot. Franklin concluded that if charge is distributed only at the external surface of an electricized conductor, then the degree of electrification decreases with an increasing external surface. This was observed in fact when the experiment was carried out.

I recounted these two experiments not only because they are striking in their simplicity but also because they are the most fundamental in their results. Franklin described all his experiments in his letters to his friend Collinson in England.

In these letters there are described a large number of various experiments which have become classical ones: the production of an electric wind, the properties of the flow of charges out of a spike, and so on. In the same letters Franklin gave in terms of his hypothesis a correct explanation of a number of already known electric phenomena, for example a picture of the accumulation of electric charges in the Leyden jar, and based on this he constructed a plane condenser. Collinson reported the studies of Franklin at the Royal Society. Afterwards he published them in the form of a particular book which represents the basic scientific work of Franklin. The book ran into a number of editions and has been translated into many languages.

I shall not describe other experiments of Franklin, and shall mention only experiments proving the electric nature of lightning. These experiments became famous already in Franklin's life-time and made him illustrious. Although the hypothesis that lightning and the discharge obtained from electricity produced by rubbing are the same phenomenon though on a different scale was put forward already before Franklin, no experimental evidence of the validity of this hypothesis was available.

The clearness and accuracy of Franklin's concepts of electric phenomena allowed him to devise an experiment which for the first time proved conclusively the electric nature of thunder discharges. The idea of the Franklin experiment consisted in the following.

Assume that a long vertical metal rod insulated from earth is placed between a storm cloud and earth. If the storm cloud has an electric charge, then a charge of the opposite sign is supplied to the upper part of the rod. If a spike is put at this upper end of the rod, then the supplied charge will flow out and the rod will get charged by electricity of the same sign as the storm cloud.

Franklin supposed that the presence of this charge could be detected from a spark arising when the conductor is touched by the free end of a filament connected to earth. Franklin's assumption that the rod should be placed on a hill nearer to the cloud in order that the experiment may succeed turned out to be wrong. Since no such hill was present near his house, he thought that he would not succeed in carrying out this experiment. He described in detail how it should be carried out and suggested that somebody else might perform it. As to himself, he decided to carry out an analogous experiment but in a somewhat different way needing no hill.

For this experiment, instead of a metal rod he decided to use a string, lifting it by a kite. Since a thunderstorm is always accompanied by wind one can fly the kite and since a thunderstorm is also accompanied by rain the string getting wet becomes a conductor and can replace a metal rod. In order that the string might be charged easily its upper end was provided with a possibility for the induced charge to flow out. For this purpose Franklin put spikes at the corners of the kite frame. In order to insulate the string from earth a silk band protected from rain was tied to the lower part of the string. A metal key, from which Franklin obtained a spark during a storm, was suspended on the end of the string near ground. In such a way he proved in the presence of his friends and acquaintances the electric nature of the thunder discharge. The experiment with the kite was performed by Franklin on the 12th April 1753; he was then the first to reveal that storm-clouds are as a rule charged negatively.

The French scientist Dalibard constructed exactly according to Franklin's description an insulated rod at Marley, and on the 10th May 1752 in the course of a storm he obtained experimentally for the first time the electric sparks, and thus the electric nature of storm was proved successfully somewhat before Franklin but by his method.

The technical details of these experiments of Franklin as well as of other ones are very interesting, because they show his great experimental inventiveness. In studying the history of the development of Franklin's studies one is astonished by the quickness with which Franklin's concepts entered science. Despite the opposition of a number of eminent scientists, for example, Abbot Nolle and Wilson, Franklin's ideas in a very short time got inculcated upon science. Of course, scientific truth is a single one and will always make its way through, but it depends on men and not on the truth whether this way will be a rapid and straight one. In this respect Franklin's activity can also nowadays be quoted, speaking in contemporary language, as an example of how one has to inculcate one's scientific achievements.

Franklin attempted to make each of his researches at once a property of the largest possible circle of men. At his home in Philadelphia he organized a Philosophical Society from the local citizens, where he carried out demonstrations and read lectures. He often stayed abroad, where he made wide contact with the scientific community. He kept an intense scientific correspondence with a number of leading French, Italian and English scientists even when America was at war with England. He taught himself the French, Italian and Spanish languages, and knew also Latin.

His ability of struggling for new ideas manifested itself particularly clearly when he had to inculcate the lightning conductor upon life. But we shall speak about it later on. We return now to the further development of Franklin's research on electricity. In connection with Franklin's ideas scientists from many countries got engaged in experimental studies on the nature of electricity. In our country, at Petersburg, Lomonosov and Richman constructed rods for the study of atmospheric electricity, and called them "the thunder machine".

Unfortunately, although the studies of Lomonosov not only in the field of electricity but mainly in the field of chemistry where he first discovered the law of conservation of matter were of fundamental importance, they could not influence the development of international science to an extent that they undoubtedly deserved.

I think that the main reason for this is lying in the fact that social conditions in which Lomonosov lived and created did not allow him to make contact with scientists from other countries and to stay abroad. The solitary work of Lomonosov and Richman undoubtedly hindered also an effect of the Russian science on the international one.

The fate of Richman was particularly sad. In his studies Richman rightly pointed out that a further development of Franklin's experimental research should proceed in the sense of finding a quantitative description of the electric phenomena.

Searching for a method of quantitative measuring the charge of the electricized rod of a "thunder machine" in the course of a storm, Richman imprudently bent down and approached too closely the conductor, wishing to make a quantitative record. He was killed on the spot by an electric discharge through his head. This happened in 1753.

After Franklin's work the greatest step in the development of the science on electricity was the transition to a quantitative description of electric phenomena. This was done by Coulomb and not earlier than in 1785. It is well known how he with his torsion balance discovered his fundamental law of interaction of electric charges. Coulomb found that the strength of interaction depends on the square of the distance between the charges.

The subsequent theoretical studies of Gauss, Laplace and Poisson developed this basic law of nature into a consistent theory of the electrostatic field which is nowadays so widely applied. But in the history of the development of the teaching on the electric field there is a relatively little known page referring to fundamental studies of Franklin, which is interesting to note.

Nearly 100 years after the work of Coulomb, in 1877, Maxwell published an article on unpublished studies of Henry Cavendish in the field of electricity. Maxwell as the first director of the Cavendish laboratory at Cambridge was given the archives of Henry Cavendish. In these archives he discovered a manuscript of Cavendish completely ready for publication proving experimentally the square law discovered by Coulomb. The experimental evidence in the work of Cavendish differed essentially from that of Coulomb; the method was more simple and the evidence more accurate than those by Coulomb. There was no date on the Cavendish manuscript, but Maxwell believed it to date at any rate not later than from 1775, consequently at least by ten years before the discovery of the Coulomb law.

In his studies Cavendish started from the fact that it can be theoretically shown that on a cored metal conductor the total electric charge can be distributed at the external surface only if these charges are repelled by one another according to the law of the square of the distance. But the experimental evidence of the charge distribution at the external surface of the conductor was already given by Franklin in the experiment with the electricized tea-pot and the chain, about which I have already spoken; it was necessary only to find a way of making this evidence more accurate.

Therefore Cavendish carried out the experiment in a more exact way. Instead of a tea-pot he used a hollow metal sphere, and instead of a chain he put another concentric sphere inside the first one. Both spheres could be either insulated or earthed, according to what was needed. Cavendish chose concentric spheres because this form made it possible to elaborate quantitatively the obtained experimental result. The Cavendish experiment consisted in providing evidence that the charge imparted to the external sphere was distributed only over it and did not pass on to the internal sphere.

Maxwell organized at Cambridge a reproduction of the Cavendish experiment but with a more perfect measuring device, and showed the Coulomb law to be accurate to within nearly one part in a million, whereas by the method of Coulomb's torsion balance this law could be checked with an accuracy to within not much more than one percent.

Here the question arises why in the course of 100 years the first-class scientists such as Gauss, Poisson, Laplace and so on, the founders of the theory of electric field, had not noted that the simple Franklin experiment with the teapot could already serve as an experimental evidence of the validity of the Coulomb law—one of the fundamental laws of the electrostatic field?

How could it happen that Cavendish's research remained for 100 years unknown? Maxwell in his article also pointed out that in this Cavendish study ready for publication there was beside the Coulomb law also formulated and roughly checked the Ohm law! And this was made 70 years before Ohm himself discovered this law!

It is natural to put the question how it could happen that such a great scientist as Cavendish, who was called by many people the "Newton of contemporary chemistry" could leave unpublished this work on electricity, which he surely could not but consider as a fundamental one?

History will hardly ever find an answer to this question, but it is most likely that Cavendish simply forgot to send it to the press.

This explanation seems at first to be unlikely because, one would think, his friends were bound to know these studies and remind him of them. But here lies the peculiarity in the character of Cavendish—he had no friends and no colleagues, and in general avoided people. He was a very rich man, brother of the Duke of Devonshire, his way of life was an exceptionally unsocial one, and he took interest only in science. Even his household servants were not allowed to appear in his chambers in his presence. His meal was served on the table before he came into the dining-room. Due to this isolation from people the scientific work of Cavendish, the results of his greatest scientific achievements made in England, had no effect on the development of international science.

Much later French scientists discovered independently these laws of nature. They transferred their knowledge to people and these fundamental laws of nature are called justifiably by the name of Coulomb and Ohm.

Besides purely scientific studies, we owe to Franklin a universally recognized achievement—his invention of the lightning conductor. The history of inculcation of this invention upon life is also very instructive. This is a long story and many studies are devoted to it. Therefore I will only briefly recount how Franklin invented and inculcated the lightning conductor.

I have already mentioned that Franklin proved lightning to be none other than an electric spark arising between clouds and the earth when these are of opposite charge. After the essence of thunder discharge was elucidated there naturally arose the question how one could rationally combat destructions and fire caused by lightning. It became clear that when a building, ship or any other object is struck by lightning then the damage is due to the fact that an intense electric current passing over poorly conducting surface produces demolitions and ignitions. Hence, if the electric discharge arising when a building is struck by lightning is allowed to pass over a well-conducting medium like metal, then no destruction will be produced. It became understandable why buildings with metal roofs and sewers were less endangered by thunder discharges. For instance, the Solomon temple in Jerusalem was not once in the course of a thousand years endangered by lightning, since it was covered by polished metal plates.

It is natural that after Franklin's studies revealing the nature of thunder discharges, a number of persons arrived at the idea of the possibility of protection from lightning by removing electric discharge through well-conducting metal rods.

It is not unlikely that a small priest called Prokop Divish, starting from the understanding of the processes of electric discharges and using as the conductor for removing the current a chain connected to ground, established independently on the roof of his house in a small provincial town in Czechoslovakia in 1754 an arrangement resembling closely Franklin's lightning conductor. This undertaking ended sadly, because the inhabitants of the little town, seized by a superstitious fear, tore down and destroyed the arrangement.

Undoubtedly, Franklin with his sharp practical intelligence saw before anybody else the possibility for the protection against lightning by removing the current. But it was much more difficult for him to find the most rational form of lightning conductor and to make the public opinion recognize it as an efficient means of combating demolitions caused by thunder storms. He managed this task excellently, and his activity in this sense can also nowadays serve as an example how new technological ideas are to be brought into practice.

Franklin not only took out no patent for his lightning conductor but allowed gratuitously everybody who wanted it to take advantage of it. Moreover, he carried on a large and skilful campaign to inculcate it upon life. For lack of time I cannot recount completely the story of the inculcation of the lightning conductor, hence I shall dwell on the most striking moment.

It may well be that no other invention gave rise to a storm of various objections such as raised 200 years ago by that small metal rod which nowadays crowns almost every building and is a standard element of its construction.

Two hundred years ago the objections raised against lightning conductors were of extremely diverse origins; there were also such arguments: "lightning is a tool of punishment in the hands of Providence, hence it is a sin to oppose it". Another not less "convincing" argument was the following: "thunderstorms occur when evil demons get out of the obedience to God". Therefore, the single right way of combating it was assumed to be the tinkling of bells which fought back the evil demons. That is why for a long time it was assumed to be necessary to ring bells during a storm. Since it is obvious that church bell towers are the most vulnerable to lightning strokes, it was rather dangerous to ring the bells during a storm. Even after the invention of the lightning conductor they were for a long time not set in churches, and one used as before to tinkle. One hundred and twenty bell-ringers were killed and 400 bell towers were destroyed in Germany within 30 years at the close of the eighteenth century.

But Franklin concentrated the main struggle for the lightning conductor not on superstitious-religious objections arising from less-educated sections of the population. The struggle was carried on against the very top of the society of that time. Both scientific and political objections were being raised against lightning conductors.

When Franklin gave the description of the lightning conductor he pointed not only to its obvious function in providing a free path to the electric current over the metal rod into earth, but also to a possibility for the existence of another process.

Franklin assumed that if a cloud-storm is above a building, and if the lightning conductor is provided with a spike, then the electric charge may flow slowly out of it. We now call this phenomenon the silent discharge. It is this phenomenon that will neutralize the charge of the cloud and discharge it. Therefore Franklin supposed that a lightning conductor not only protects buildings but may prevent thunder discharges. Scientific opponents of Franklin supposed on their part that the flow of the charge out of the spike would not only not neutralize the charge of the cloud but would even produce more favourable conditions for the occurrence of lightning. Hence in their opinion a lightning conductor was rather harmful, since it made possible the occurrence of thunder discharges which would not at all take place if the lightning conductor were absent.

Scientists who adopted this standpoint assumed it to be particularly harmful and dangerous for a building to be in the neighbourhood of another one provided with a lightning conductor.

Public opinion was very much interested in these problems, and this is well illustrated by the known case when at Saint-Homer, France, Monsieur de Vissery installed a lightning conductor on his house, and his neighbours were so much frightened by and indignated at this that they brought an action against him. The trial raised clamour and lasted for several years between 1780 and 1784. It is interesting that the then young lawyer Maximilien Robespierre undertook the defence of the lightning conductor, and this famous affair marked the beginning of his reputation. It is also curious that Marat, who considered the lightning conductor to be dangerous and was against its establishment, was one of the experts on the part of the plaintiff. After a long struggle and many appeals against the court's decision de Vissery finally won his case.

Franklin's tactics in this struggle for the lightning conductor is also interesting.

He as a rule did not appear in public, but by means of his discussions and his extensive correspondence he continuously influenced leading scientists and public figures. Through such a propaganda he created a powerful group of partisans from progressive-minded persons of that time, which struggled to bring into practice his invention—the lightning conductor.

In England the struggle for the lightning conductor assumed a political character. The English scientist Wilson attempted to prove that it is possible to prevent the harmful effect of a lightning conductor if one makes its end blunt and thus hinders the flow of charge. Since the time of this debate coincided with the epoch of America's liberation from colonial conditions so that Franklin became a great political figure of the new America and one of the most agile fighters for freedom, each English citizen who provided his lightning conductor with a spike and not with a blunt end was considered as politically suspect.

The King of England, George III, asked the Royal Society to retract its decision in favour of the spike on Franklin's lightning conductor. Sir John Pringle, who was then the President of the Royal Society, King's Physician in ordinary and personal friend of Franklin, gave the following famous reply to the king's demand: "I will always do my best to fulfill the wishes of His Majesty, but I am able to change neither the laws of nature nor the effect of its forces". For these words he was dismissed from the post of King's Physician and from the presidency of the Royal Society.

In the course of the struggle on the problem of the lightning conductor all methods were used: calumnies, insinuations, both against Franklin and his friends. Franklin remained calm, did not pay attention to the personal attacks and constantly stated that in problems of science the truth is revealed only by experiment.

Indeed, it was experiment that resolved this controversy, but after several dozen years, when the teaching on gas discharges and on the electrostatic field reached the contemporary level. At present we know that this controversy was groundless, since for an ordinary lightning conductor it is of no importance whether it is provided with a spike or a blunt end. At a small distance from earth the geometric form of the end of the lightning conductor cannot have an appreciable effect on the electric-field distribution above ground.

However, Dr. Schonland, one of the leading experts on thunder discharges, has pointed out that the process of neutralization of the cloud charge by means of a silent discharge, predicted by Franklin, can nevertheless take place, but only if the spike of the lightning conductor is at a large distance from earth comparable with the altitude of the cloud. This is the case with lightning conductors placed on the loftiest American sky-scrapers; then one can indeed observe from the spike of the rod a silent discharge which does not go over into lightning. Schonland added that this would undoubtedly produce in Franklin a feeling of fair satisfaction if he could know of this.

Nowadays the lightning conductor is an integral part of all buildings, and there are surely innumerable edifices, installations and ships which were preserved by it from demolition and fire. The merit is justly ascribed to Franklin's initiative.

I shall only briefly present Franklin's activity in other branches of science, because besides the described famous achievements Franklin has also achievements in other fields.

Franklin took interest also in geophysics, gave a map of the Gulfstream, invented a musical instrument with shaking glass balls, an economic stove which is also at present widely used in America and France, street lamps, double spectacles for presbyopia, and many other things. Moreover, owing to his sociable character and lively mind Franklin gave much advice and furthered the development of science. Of course, data on the majority of these advices sank into oblivion, but some reached us.

Thus, for example, Louis XVI asked Franklin to become a member of a committee of inquiry on the validity of a medical treatment proposed by Dr. Mesmer who made use of so-called "life magnetism". It is interesting that the well-known doctor Guillotin, the inventor of the guillotine, took part in this committee. Franklin denied the existence of life magnetism, but considered this treatment to be harmless, since it distracted wealthy people without prejudice to their health, which cannot always be said of other unsubstantiated medical treatments.

Franklin regarded with approval the flights of the brothers Montgolfier.

One should also note a field of Franklin's activity associated with possibilities of developing international science, which Franklin disposed of as a great statesman of his time.

Franklin considered scientific achievements to be the property of the whole of humanity, and the concern for the development of international science to be above political and military conflicts between different peoples. Thus, at the time of the war against England, when the famous explorer Captain Cook was coming back from the circumnavigation of the world, Franklin gave instructions to all American ships and corsairs to treat Cook respectfully wherever they come across him during his voyage. For our time it is also of interest that Franklin, taking part in the Congress, persuaded it that the embargo put on all goods of English provenance should not be extended to scientific facilities.

In studying the biography of Franklin, one is understanding more and more fully why this great man, whom the people of America gave to humanity, is universally esteemed and worshipped. Of him, Turgot—quoting Manilius—said: "*Eripuit caelo fulmen, mox sceptra tyrannis.*"

In an epoch of rapid progress of natural sciences every country gave its own great father of science—in our country this was Lomonosov, in England—Newton, in Italy—Galileo, in Holland—Huygens, in France—Descartes, in Germany—Leibnitz, in America—Franklin.

And we, the Soviet people, are grateful to the American people who brought up and gave to humanity the great Franklin.

21. HOW IS ATOMIC WAR TO BE PREVENTED?

1. Avoid the Use of Nuclear Weapons! (Lord Russell)

The destructive force of atomic weapons has reached the stage when it has become clear one has to avoid their use at any price. On this count general agreement exists; the disagreements touch upon how to achieve this desired aim. There is, however, one step about which there should be no difficulty in reaching agreement. This is the halting of test explosions similar to those which are being carried out by America, Great Britain and the Soviet Union. Test explosions must not be kept secret, and this makes agreement easier. Here there will be no difficulty in struggling with the difficult problems of inspection which soon arise when further steps are proposed.

Many would prefer the Great Powers to renounce the use of nuclear weapons and to do away with the stocks of existing weapons. But as yet mutual suspicions make it doubtful whether such an agreement would confer any benefit. Each side would suspect the other of unloyal fulfilment of the agreement. Even though unjustified, such suspicions would maintain tension in relations between East and West, a reduction of which is wanted by every rational person. Moreover, neither side would consider itself bound by the agreement in the event of war occurring. Consequently, nuclear arms, even if non-existent at the start of the war, all the same will be produced and used by both sides in the course of hostilities. From this consideration I suppose that the only means of preventing *nuclear* war is to prevent *war*.

Recently, enormous relaxation of tension has taken place in the relations between East and West, and one wants to hope that this will continue until the danger of war is made quite insignificant. Then it will become possible to clear away the controversial questions by negotiations, since both sides recognize that it is impossible to find the solution by military means. In the amicable atmosphere which will arise as a result, the renunciation of the nuclear arm will be sincere and no doubts will be left on either side.

I would like to see an international tribunal set up to which peoples would submit their disagreements. This tribunal should consist of an equal number of representatives of East and West and—for balance—representatives of unaligned countries. If such a tribunal existed, and its authority were recognized by all the Great Powers, peace in the world would be ensured.

But I do not think—and I want to repeat this—that the banning of one or

Как предотвратить атомную войну; 1. Б. Рассел (Bertrand Russell), Избежать применения ядерного оружия; 2. П. Капица, Задача всего передового человечества, *Новой Время* **14**, 39, 8 (1956).

another weapon will confer any great benefit so long as the danger of war exists. Banning could only be effective in conjunction with strict measures of inspection, to which neither side wants to agree. Moreover, even if banning were effective in peacetime, it would cease to be so immediately after the start of a war. I will go still further and say that the confidence engendered by banning nuclear arms would reduce the fear of war, and this would make war more probable.

In my view the moral to be drawn from the fact of the destructive force of modern weapons is that both sides must understand that their aims can no longer be achieved by means of war. This implies that in settling controversial questions preference has to be given to political and diplomatic methods, and not to threats of using force. This a long time ago became the rule in the internal affairs of states (excluding, of course, revolutions). It is required to extend to international affairs those methods which have proved their effectiveness in the internal affairs of peoples. Nothing, moreover, can prevent war once and for always. Whatever bans existed on paper, a big war will inevitably call forth the use of all the most destructive kinds of weapons which are only known to science, though their use will bring mankind to catastrophe.

From the moment of the emergence of organized states all diplomacy has been founded on open or concealed threats of war. It is not easy for one who has gone through this old school to adapt to the new requirements of the more dangerous world in which we have to live. But however difficult it might be for state officials to pick up the new way of thinking and acting, they have to learn this—and fast!—if mankind intends to continue its existence.

2. THE TASK OF ALL PROGRESSIVE MANKIND (P. KAPITZA)

Military experts consider that the damage which the exploding of a thermonuclear bomb brings is quantitatively so much greater than that from all previous weapons of destruction, that atomic war assumes a qualitatively new aspect.

It is beyond all doubt that if an atomic war occurs, the damage and misery caused to the people in the warring, as well as the neutral countries, will be so great as to be incomparable with any one of the most terrible disasters which have ever afflicted peoples. That is why the progressive part of all mankind must spare no effort or energy to fight the possibility of an atomic war.

In recent years Bertrand Russell has been fighting vigorously in this direction. This activity has my profound sympathy. The great authority of Russell as a scientist and thinker cannot help but have considerable influence in the fight to prevent atomic war.

A sa scientist working neither in nuclear physics, nor in its application (though often in the foreign press, quite incorrectly, this kind of activity has been ascribed to me), I also would like to make my contribution to solving the problem of preventing atomic war. Therefore, I have gladly accepted the suggestion of the editorial staff of *New Times* to speak my thoughts as regards the article of Bertrand Russell.

It is a very difficult task to find social and state measures which are able completely to safeguard mankind from atomic war. A successful solution can only be found by free and sincere discussion in the wide international framework. The problem is still further complicated by the fact that political and social, as well as scientific elements, enter into it. Such work therefore requires close collaboration between scientists and public figures. Unfortunately such collaboration has still not become part of the tradition by which state problems are solved. It seems to me that in this particular case scientists might not confine themselves solely to the scientific aspect of the problem, but it is necessary to become involved in the social and political side of the problem. If some amateurishness is revealed, this ought not to be feared since there are very few to be found who could thoroughly grasp all the facets of such a complicated problem.

In searching for the solution scientists, above all, ought to provide a clear reply as to the consequences of the damage from using nuclear weapons. The destructive action of any type of bomb on military and civil installations can be established exactly. However great they might have been, experience of previous wars shows that after one generation a country can always restore the material basis which is necessary to it for normal life. But the chief horror of the nuclear arm lies in the exceptionally severe after-effects on people due to pollution of the atmosphere by slowly decaying radio-active substances.

For a long time the many very severe forms of after-effect due to radioactive poisoning have been known. But, despite this, it is very difficult, and well-nigh impossible, to foresee exactly their biological effect on mankind after an atomic war. Modern biology bases its conclusions on empirical data obtained from statistical treatment of clinical tests. The amount of test data, as far as people are concerned, is very limited and it is very difficult to make reliable statistical generalizations. Therefore much time will pass before such important problems are satisfactorily solved as the ability of organisms to adapt themselves to radioactive radiation, or the possibility of using medicinal means for protecting people from radiation.

It is still more difficult to solve exactly the problem of the pernicious effect of radiation on heredity—to what extent it can lead to an inferior posterity. From the available scientific data published to date one may infer that most scientists consider that the poisoning of the atmosphere even after the largest atomic bomb will not lead to the cessation of life on the Earth. Moreover, one should not forget that, as the history of mankind shows, even in epidemics of the most horrible diseases there have always been people who have had a natural immunity, and have not perished or lost their vital activity.

That is why it would not be true to assert that fear alone of the severe consequences of an atmoic war will preclude the possibility of someone appearing with the way of thinking and moral level of Hitler who, to carry out his aggressive acts, would not be able to induce his loyal subjects to disregard the disasters of atomic war.

In the fight to prevent atomic war one has also to consider the possibility that full protection from nuclear weapons will be found. If this is achieved by a country with aggressive designs, by being itself immune from the direct effect of the nuclear weapons, it can decide to unleash an atomic war rather more easily.

It is therefore very important for scientists to answer the question whether it is possible to find effective protection from nuclear weapons.

History shows that any new weapon of attack always produces a corresponding new defensive weapon. It is hard to presume that the nuclear weapon is an exception from this rule.

Doubtless in different countries scientists and engineers are working intensely on this problem. The results, of course, are not made public, but all the same one can form presumptions concerning the long-term prospects.

At present it is presumed that the only possible means of protection consists in intercepting the missiles carrying the nuclear bombs. Can one assert that it is impossible to find means of completely obstructing the access to a given territory of any type of missiles carrying nuclear bombs? I think, on the basis of the state of science to-day, that it is impossible to deny the possibility of finding such a method, although it is a very difficult scientific and engineering problem which still has no obvious and generally acknowledged solution. If the problem does become solved, then in all probability it will be based on natural phenomena which either have still hardly been studied, or are as yet still unknown. Doubtless the method must also be connected with great "energetics" (power) processes, so that considerable material means are required to implement it. Consequently, work of this kind will be difficult to conceal from the "watchful eye" of neighbours.

If countries which are inclined to conduct their foreign policy "from a position of strength" are the first to find an effective means of defence from the nuclear arm, they can "forget about" any of their obligations. Either they may unleash an atomic war, or in any event use their advantage to impose their will on others.

Therefore in concluding international agreements which have the aim of preventing atomic war, it is necessary to take into account the possibility of inventing an effective means of defence. In such agreements it is necessary to provide for mutual notification of the course of experimental work on defensive measures, though this may complicate the conditions of mutual supervision over fulfilment of the agreement.

Of course, Bertrand Russell is quite right that the most reliable way of preventing nuclear war is to prevent war in general.

The basis of the fight for peace is now the natural feeling of aversion for war and yearning for peace which resides in the broad masses of working people. Therefore one ought, in every possible way, to welcome the activity of fighters for peace in all countries which stimulates, unifies and organizes this yearning for peace and prevents war arising. Unfortunately, this kind of activity by itself still cannot reliably and finally prevent war between countries. Doubtless the emotional element plays an important part in human history, but not the decisive part.

It should not be forgotten that wars are often a recurring phenomenon which inevitably has associations with social processes in conformity with natural laws.

The facts which precondition the "escalation" into war yield to scientific analysis, therefore they can be elucidated. The main task of scientists is to study these facts and in this way to assist in finding the measures which prevent war arising. The main task of public figures is vigorously to put such measures into practice.

The elucidating and eradicating of the many social factors which precondition wars ought therefore to be fundamental in the fight for preventing atomic war. Doubtless the process of eradication is already underway, but, like any social process involving the harmonious activity of a great many persons, it takes place slowly.

The danger also exists that this normalizing process of developing peaceful relations between countries can be interrupted by a war which breaks out suddenly. With all the imperfection of the forms of inspection, measures to halt the testing of nuclear bombs, to ban their production and use, and any other agreements on disarmament should therefore be welcomed and supported in every possible way. At this particular stage they are, unconditionally, the main international state measures which will make the arising of war difficult.

From these considerations I cannot agree with Bertrand Russell in his critique of proposals on banning nuclear weapons, and I would like to remind him of the saying that "le mieux est l'ennemi du bon".

22. SOME ASPECTS OF THE ORGANIZATION OF SCIENTIFIC WORK

During the forty years of development of our socialist economy the planning system has demonstrated its strength in peacetime and in war. The system of planning, which embraces the whole economy of the country, is only possible in a country where the means of production are possessed by the people itself. This is the socialist foundation of our state system, and it is a stable one with all the structural re-organization of the economy and administrative apparatus which is now taking place.

In consonance with our natural growth, two main factors will always necessitate economic re-organization. Firstly—the continuous cultural growth of the population; secondly—the continuing economic growth on an ever larger scale.

The cultural growth of working people, engineers and state and administrative personnel is providing greater scope to give them more independence in their work. Naturally, the more independence and initiative in a person's work, the greater is the productivity compared with a worker who according to his cultural level can only work well when carrying out detailed instructions elaborated above. Thus, according to the extent of our cultural growth, the role of the central economic and administrative machinery will diminish.

In parallel with the continuous cultural growth of working people the wealth of our country is increasing also. During the last forty years the economy has grown greatly, now we have several very large and advanced metallurgical, manufacturing, agricultural and other centres in the country such as are able to go on expanding quite independently. Experience shows that with the large scales of production that we have achieved, even with modern means of communication, petty administrative tutelage of such regions from the centre interferes with their natural growth.

It is a principle of socialist economic activity that with the removal of administrative tutelage there should always be a single plan for the management and development of the economy of the country as a whole. With cultural growth of the country this plan must become more thoroughly scientifically substantiated and be carried out rigorously according to quantitative and qualitative indices.

In his theses Comrade Khrushchev proposes to introduce the principle of democratic centralism more deeply into the control of economic activity, whilst keeping to strict planning, this being the natural corollary of the continuous

П. Л. Капица, Некоторые вопросы организации научной работы, *Правда*, 4. v. 1957.
P. L. Kapitza, The organization of scientific work, *World of Science* 1, 5, 14 (1959).

rise in the cultural level of the working people and of the increasing scale of production in the national economy.

On the other hand it is known that the increase in the rate of output per man and greater effectiveness of production are due not only to the development of organizational forms of economic control, but also to a large extent to success in putting scientific achievements into effect.

It seems to me that with the cultural growth and increase in our economic wealth over the past forty years we are facing not only the problem of finding new organizational forms for planned development of our national economy, but also that of more effective organizational forms for the development of front-rank science and putting its achievements into effect.

A study of the organizational forms of scientific work, both with us, and also abroad, shows that the cultural growth of a country and a larger scale of its economic activity, also involves evolution of the forms of organization, and this has to be borne in mind in the forthcoming re-organization of the administration of industry and building.

It is clear that the organizing of the solution of great scientific and technical problems, and success in putting the results into practice, not only requires very great resources, but also entails the involvement of many different specialists. In the U.S.A., Great Britain and in other capitalist countries, industry has grown so much that large firms and combines have been compelled to solve scientific and technical problems by organizing their own science laboratories and institutes.

Our Ministries are essentially even larger industrial Trusts, therefore they too have been compelled to set up their own research institutes and project-design offices, to involve our leading scientific institutions and to organize the solving of great scientific-technical problems in the same way. In our case the Ministries are also able to ensure that the results of scientific research are adopted in enterprises under their control.

Clearly, the bigger the Ministry, the bigger are the projects that can be elaborated and adopted. One has to face the fact that in regionalizing industry this advantage will largely be lost. The large industrial Ministry-type Trusts will cease to exist, and the regional Trusts will be on a smaller scale. Therefore, to retain the advantage of centralized direction in solving new technical problems and putting them into effect, new organizational forms are required for centralized direction.

The *a priori* existence of such new forms is of course quite possible since the main advantages of centralized direction in socialist construction remain steady. The master in our country is one and the same in all the regions—it is the people. Also a single plan will hold for economic and cultural construction.

The Engineering-technical Committee, the new organization proposed in the theses of N. S. Khrushchev, obviously, must become a central directing and authoritative State body which will organize scientific research and put scientific-technical achievements into effect in all economic regions of the country. If this committee is a vigorous organization, vested with adequate powers, having large financial backing, and actually under the control of top scientists and engineers, I think that the organizational forms can be found with which the committee is able to become such a powerful organization that it will effect the link between science and practice still better than hitherto through Ministries.

In working out new organizational forms for scientific-technical research and putting the results into practice, one should bear in mind the experience gained in solving the great technical problems which have arisen in connexion with the rapid growth of science and engineering in recent years. This experience, I believe, shows that one has succeeded in solving great technical problems by scientific methods most effectively in the case when the scientific research projects have been organized in a different way than has hitherto been considered normal. Since little that is clear appears to be being said about these new organizational forms, I shall dwell on them in somewhat more detail.

Firstly, in chronological order I will briefly indicate the main phases through which scientific research has developed on the national scale.

The valuable influence of science on engineering and the economy was understood a long time ago. At first scientific research was carried on in laboratories of universities or similar higher educational establishments. At the end of the last century scientific research had already developed to such a scale that independent laboratories and research institutes started to be set up. One should mention that in these institutes and laboratories research was organized according to definite fields of knowledge: physics, chemistry, biology, astronomy and so on. Then the need arose for large scientific institutes of a more specialized nature, for instance, Institutes of acoustics, crystallography etc., started to be separated off from Physics Institutes, and physical chemistry, organic chemistry etc., from chemistry.

The need for a closer link between science and practice was felt at the beginning of this century and, more particularly, after World War I. Then in industry large iron and steel works, electrical engineering works and other plants began to organize their own factory laboratories in which current problems were solved connected with the adoption of scientific-technical achievements. The need for further growth of these factory laboratories produced large independent scientific institutes from them working in particular fields of engineering. Abroad they are to be found in large industrial combines, whilst with us they are usually with the Ministries. We call them industrial departmental institutes.*

In scientific institutions and these institutes in any cultured country the personnel numbers many thousands and here the staffing is from the more gifted and capable persons. Large sums are spent on the needs of these institutions, which are continually growing in number and size.

The characteristic feature of scientific and industrial departmental institutes is the organization of their research in particular fields of knowledge, and until recently this form of organization has satisfied the demands of practice.

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* In the Soviet planning system at this time a Ministry was set up for each main industry; but because the word "industrial" can apply to an industry as well as to industry in general, the word "departmental" is used to denote a "branch of industry", i.e., an industry (*Translator*).

But during the last ten years life has necessitated more and more the solving of great scientific and scientific-technical problems which cover many fields of knowledge all at once. Take, for instance, the construction of an aeroplane propelled by atomic energy. For its design physicists are necessary—atomic scientists, thermotechnical specialists, experts in aerodynamics, to say nothing of the aircraft designers, metallurgists etc.

Such problems we say are composite. But if one analyses the problems now facing science and practice, it appears that with few exceptions all these problems have to be considered composite in some measure or other. It is obvious that the solution of composite problems is not within the capacity of a single specialized institute. Usually with us they are solved in the following way. Always there are some people who are most interested. They take upon themselves the initiative to direct and organize the work. This is allocated out as separate tasks to different institutes, design offices and other similar institutions.

When the backing of central directive bodies has been present, this system of organization has been possible with us. But it is not hard to see that all the same the system is not only cumbersome, but also that it has grave defects. Its main drawback is the lack of coordination, the lack of constant personal contact between the persons working. In such conditions the enthusiasm and sense of purpose are lacking which are so necessary for intensive creative work.

After the reform which is proposed to be carried out, and with the liquidating of some of the Ministries, the lack of central direction for the departmental institutes and design offices, it will be still more difficult to organize by such a method the solution of complex problems and put them into effect.

Life is prompting how necessary it is to find an escape from this difficulty. Many instances show that whenever in solving composite scientific or scientific-technical problems a single independent organization has been set up consisting of different specialized scientists and engineers, but all pursuing a common aim, i.e. to solve the scientific-technical problem facing them and put the results into effect, such organizations have always operated successfully. One example is the organization which we set up for solving the problem of oxygen intensification of metallurgical and other processes. True, in this respect life itself has had problems on a similar scale solved by special independent organizations. But, essentially, nothing prevents this method of organization also being extended to the solution of problems connected with semiconductors, heat-resistant steels, polymers and so on.

Therefore, parallel with our present thematic scientific research institutes and our design offices functioning in each field of knowledge, it is becoming necessary for us to begin to set up Specialproblem scientific-research organizations on a large and small scale. Each such organization will serve for solving a definite specified urgent problem. Depending on their structure, these organizations may, if necessary, incorporate their own scientific-research institutes, laboratories, design offices, pilot factories, etc.

For every such organization to be successful, they need to be regarded as permanent institutions, though set up only for the duration of the problem, be it several months or several years. Since these organizations will be of a temporary nature, if necessary, they can easily be organized in any district wherever the problem is most pressing and closely associated with the demands of practice.

This way of solving scientific-technical problems will not require greater resources. However, the main difficulty will be the need to re-educate some of our scientific-technical workers, who are usually striving to stabilize the work of their scientific institutions, and fear having to tear themselves away from them. It is necessary to instil and encourage in scientists and in progressive engineers the feeling for mobility. Such an organization for solving scientific-technical problems may be represented figuratively as a mobile battle formation made up of military units with different kinds of weapons. To this formation definite operational orders are given, and when carried out the corps is re-organized for its next task.

Naturally, the creation and direction of such organizations for solving definite problems in engineering should be taken up by the Engineering-technical Committee and this should be regarded as one of the most fundamental tasks.

I believe that after the abolition of some of the Ministries and after the organization of economic regions it will be much easier for the Engineering-technical Committee to organize and supervise the development and adoption of complex scientific-technical problems when the Specialproblem scientific organizations will have been created, than to do this as formerly with the work split between thematic institutes, design offices and factories.

Naturally, parallel with these Specialproblem scientific organizations, earlier thematic institutes like the Institutes of the Academy of Sciences, and the industrial departmental institutes of large works and administrations, should still continue.

The new scientific-technical organizations should relieve our thematic institutes both of alien thematics and from overloading by cadres. This will give them the opportunity to concentrate on the solution of problems of great scientific value.

I think that very probably the principle of organizing work according to problems will in the future have to be at the basis not only of solving applied scientific-technical problems, but also in solving theoretical scientific problems, and it must have an influence on the organization of scientific work in the Academy of Sciences. That Specialproblem organization will also be effective here is seen from the fact that today the most interesting and foremost problems in science arise when several fields of knowledge overlap, for instance, biology with chemistry and physics, or radiophysics with astronomy. The need for a composite elaboration of big scientific problems is corroborated by the recent instance of the combining of scientific research and scientists according to problem thematics at the Academy of Sciences of the U.S.S.R. This amalgamation was not only possible, it also went off with ease and was well supported by the scientific community.

It is beyond the scope of this article to elaborate in detail the actual organizational forms necessary for putting into effect the proposed principle of Specialproblem organization of scientific-technical research.

23. THE PATH OF DEVELOPMENT OF PHYSICS IN THE U.S.S.R. IN THE FORTY YEARS OF SOVIET POWER

Now that our country is commemorating the fortieth anniversary of the Great October Socialist Revolution, besides achievements in industry and agriculture, the general growth of culture and, in the first instance, science, is also being reviewed in retrospect. In assessing the level attained by the country's scientific culture the successes of Soviet physicists are exceptionally significant. Of all the experimental sciences it is physics which is discovering those laws of the properties of matter which are at the basis of the progress of the majority of natural sciences, as well as at the basis of modern engineering. Therefore the height of the level attained in the development of physics is undoubtedly an objective index of the overall growth of science and engineering. It is also well known that of all the experimental sciences modern physics requires the most complicated and expensive technical base; therefore today only a big country with a widely developed industry, which is at a high technical level, can itself ensure successful development of experimental physics.

Even to a non-specialist it is obvious that here, in the U.S.S.R., after forty years of Soviet power physics has achieved an exceptionally high level. It is generally accepted that the study of the properties of atomic nuclei is now one of the foremost fields of physics. Owing to a number of fundamental discoveries in the last decade, this field of physics today is not only developing intensively, but it is also the necessary basis for the technical use of atomic energy for peaceful purposes as well as for the purposes of defence of the country. It is well known how exceptionally successfully and rapidly Soviet technology has coped with the finding of an original solution of the problem of creating the thermonuclear weapon and with the problem of the technical use of atomic energy for peaceful purposes. Clearly, this would have been impossible if in the U.S.S.R. there had been no great forceful research in the field of modern physics.

Recently Soviet science has achieved another outstanding success—the launching of the first artificial Earth satellites. And in this achievement an essential part was played by the high level of development of our physics.

The wide scientific physical investigations in the U.S.S.R. are carried on at a large number of institutes scattered throughout the country. Our achievements in the field of theoretical physics, nuclear physics, low-temperature

Путь развития физики в СССР за сорок лет советской власти, *Журнал Зкспериментальной и Теоретической Физики*, **33**, 1081 (1957); *Soviet Phys.- JETP*, **6**, 835 (1958).

physics, optics, semiconductors, solid state physics, theory of combustion and so forth, are well known to readers of our journal from the articles published by those working at these institutes. The continuous growth of Soviet physics is reflected not only in the growth in the size of our journal, which has increased 3–4 times in the last five years, but also in the fact that the lack of space is continually necessitating the creation by the Academy of Sciences U.S.S.R. of new specialized journals in particular branches of physics, for example, crystal physics, optics, metallurgy, technical physics, acoustics, electronics and so on.

If one compares the volume of scientific work before the Revolution, forty years ago, with that being done now, it is even difficult to try to find a suitable scale for comparison. One may, for instance, recollect that all physics in pre-revolutionary years was concentrated in the laboratories of a few leading higher educational establishments. As regards the value of the equipment, all these physical laboratories taken together were only a small fraction of the cost of a modern accelerator, such as, for instance, the proton synchrotron constructed in the Joint Nuclear Research Institute at Dubna on the Volga. This remarkable apparatus was commissioned this year; in it the elementary particles are accelerated to record energies af 10 milliard electron volts.

Naturally, in the last decade large scale physical research has been growing not only in our socialist country, but also in such most developed capitalist countries as the U.S.A., Great Britain and others. It is obvious itself that in both capitalist and socialist countries the same Nature around us is being studied and therefore the same laws are found governing the physical processes taking place there. But the organization of the scientific work itself in socialist and capitalist States is quite different and with the development of the socialist economy this will stand out more and more. This difference is connected with the fact that in the socialist State science is regarded as the basis on which the material and spiritual culture of the country grows. Another corollary of this relation to science is that, firstly, it is a fundamental concern of the State to be in charge of the development of science and, secondly, the various directions and fields of science must develop, not spontaneously as in capitalist countries, but in concord with the general growth of the material and spiritual culture of the country. In the socialist state science must develop by plan and be financed from the State budget of the country according to a single plan. One should not conceal that in practice it does not appear to be so easy to find forms of organization of science in which science would develop not spontaneously, but according to a rational plan, and to find rational forms of financing scientific institutes under which a scientist would retain the creative approach to his work. One has to point out that despite the successful work of our physicists, it is becoming still more necessary to continue the work on further perfecting organizational forms of scientific work in which the scientist can labour with the greatest clarity of purpose and with maximum productivity.

In comparing the organizational forms existing for scientific work in socialist and capitalist states, the question is usually raised of the difficulty of matching the necessary freedom for effective creative work with work according to a rigidly fixed plan. In the first stage of the development of planned organizing of scientific work in the Academy of Sciences the attempt to carry it out was too strict and in too much detail, and actually these contradictions did arise. But gradually experience left them behind. The main task of the planning is becoming that of indicating those fields and directions of scientific development which the country most needs at a particular time. Now the more efficacious method of acting upon the right purposiveness in the development of science is to encourage those scientific works which exert most influence on progress in applied fields of knowledge, engineering, industry, medicine, agronomy and other main branches of the economy, or else in fundamental problems of science as a whole. More attention is paid to priorities, more funds are allocated; incentives are granted in the form of Prizes of national significance, like the Stalin and Lenin Prizes and others. The administration is increasingly endeavouring to guide the work of scientists to tackle the big problems of the economy and the basic problems of the kind common to science as a whole. Scientists are faced with broad problems such as, for instance, the production of specially pure chemical elements, high-grade heat-resistant steels, physical processes in semiconductors, transmission of power over very great distances and so on. These problems are usually composite in nature, they break down into a range of independent problems and they relate to different fields of science and engineering, but it is necessary to solve them in concord. Often it is the most difficult of these problems which fall to the physicists' lot. This entails the physicist having to concentrate on definite fields of knowledge in which the actual ways of solving the problems have to be found by him himself, and here there is plenty of room for broad personal creative initiative. Many institutes are also being drawn to the solution of a single problem, each going its own way.

Searching for the organizational forms of science as the basis for growth of the socialist planned economy is as yet far from being a finished process in the U.S.S.R. These forms of organization of science, especially in the industrial departmental institutes, are still largely determined by the structure of the administrative apparatus controlling industry. As life is showing, the structure of our economic apparatus is also continuously changing and doubtless it is travelling the road of amplifying democratic centralism, i.e. in the direction of the big re-organization of the economy taking place right now. The main idea of the present re-organization is that the central organs in the country are leaving themselves only principal management functions such as the development and introduction of new techniques and the broadly planned harmonizing of development of the whole economy of the country. Detailed economic control of industry is in the main being handed over to regional organizations. This re-organization of the economy will inevitably involve the re-organizing of scientific work. The detailed planning and organizing of the work itself will become more and more concentrated in such working bodies of scientific institutions themselves as Science Councils. But the broad planning of science which is determined by general questions of the growth of the country's socialist culture, must naturally remain centralized and rest with such institutions as the Academy of Sciences, the State Planning Authority (Gosplan) and other bodies of significance to the State as a whole. Planned organizing of science in the U.S.S.R. is already making science more purposive and leading to better utilization of scientific cadres and scientific equipment.

Comparing the link of science, and physics in particular, with the priorities in the country's life, one has to say at once that the link in the Soviet Union is already far more effective than even in the most developed of the capitalist countries. It is this which explains the high pace at which Soviet scientists and engineers succeed in solving the more difficult problems of nuclear engineering, rocket engineering and aviation, radio engineering, metallurgy, power engineering and of other fields. Although the Soviet Union still lags considerably behind the U.S.A. in the level of production of some most important kinds of output, the tempos of solving scientific-technical problems and putting the results into effect have in many cases in the U.S.S.R. broken all records and these are still held. This can be explained only by the close and friendly relations between science and engineering, which is possible in our State mainly because here under socialism there is only one sole master, and this master is the people itself. In the capitalist system where industry is in private hands and firms frequently compete with each other, scientific organizations are to a considerable extent left to their own resources and the uniting in work of all scientific forces and all industry, which is achieved naturally and simply in our socialist country, appears impossible in the capitalist system.

The tempos of growth of soviet physics, as of any field of science, also are determined by the relation with foreign science. We all consider that the main task of science is to learn to apprehend Nature, and the main aim of this knowledge is the subjugation of Nature to the good of mankind.

Scientific truth is one, and the sooner it is apprehended and the more intensively science develops, the better is this for the good of all mankind. Nowhere in the history of the development of human civilization has international co-operation conferred such great benefits on people as in the field of science.

We are very glad that during all these years our journal has not only been read by us in the Union, but it has also been widely circulated abroad. We are also glad that in recent years an English translation of the *Journal of Experimental and Theoretical Physics* has begun to circulate abroad.

International relations between scientists in their work serves not only progress in science. Mutual understanding between scientists leads to mutual understanding and confidence between countries. Without confidence between people the peaceful co-existence which is so necessary for a happy and prosperous life of people is impossible. On physicists it is incumbent, more than on any other group, to be concerned right now for peace, for nobody but they are responsible to mankind for having put in its hands the new type of weapon of destruction which can lead to disasters on an unprecedented scale if carelessly handled.

24. LOMONOSOV AND WORLD SCIENCE

INTRODUCTION

In pre-revolutionary Russia Lomonosov was always considered the founder of our science. In the Soviet Union his work and life are studied from our schooldays onwards. Beyond the boundaries of our country Lomonosov is less well-known and therefore for readers abroad this Foreword is to sketch out this great man's image, albeit in most general terms.

Lomonosov lived from 1711 to 1765 and his work followed directly upon the reign of Peter the Great (1672–1725), the great reformer of olden Russia who directed the country's cultural development along the same paths along which Europe was developing. It is generally well known that in the period before Peter the Great, Russia was for several centuries under the tsarist yoke, as is generally known, being cut off from European culture and it was a slow process to reverse this trend. With Peter the Great the process was considerably accelerated, and it is Lomonosov who is rightly considered to be the creator in Russia of an independently developing science in all its main fields. Lomonosov's scientific work in the social sphere covered an exceptionally wide front. To give one an idea of it, I shall cite just some of the principal lines which it took: he laid in Russia the basis of higher education by creating a university in Moscow. Although the Russian Academy of Sciences had also been inaugurated by Peter the Great, at first it consisted only of foreign scientists (amongst whom were such great names as Bernoulli and Euler). In the Academy of Sciences Lomonosov became the first big Russian scientist and he assisted much in its development and growth. Most of all Lomonosov paid attention to investigations into metallurgy, geology and geography and also to the study of Russia's mineral wealth. Here he himself did much original research in these fields. Lomonosov occupied himself with the history of Russia and worked on the development of its language, the vocabulary of which was still not rich enough for the development of literature and poetry. Yet in carrying out this extensive social-science work Lomonosov was also a poet and an artist. His odes and verses are read with pleasure to this day. He worked out the mosaic technique for himself (founded glass and made smalt) and himself assembled

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П. Капица, Ломоносов и мировая наука, *Известия* 18. xi. 1961.

П. Л. Капица, Ломоносов и мировая наука, *Жизнь для Науки*, Знание, Москва 1965, стр. 3.

П. Л. Капица, Ломоносов и мировая наука, *Успехи Физических Наук* 87, 155 (1965).

Text of a talk given to the Physico-Mathematical Division of the Academy of Sciences of the USSR in the Institute for Physical Problems, on 17 Nov. 1961.

PLATE 21. Mikhail Vasil'evich Lomonosov (1711–1765).

PLATE 22a.

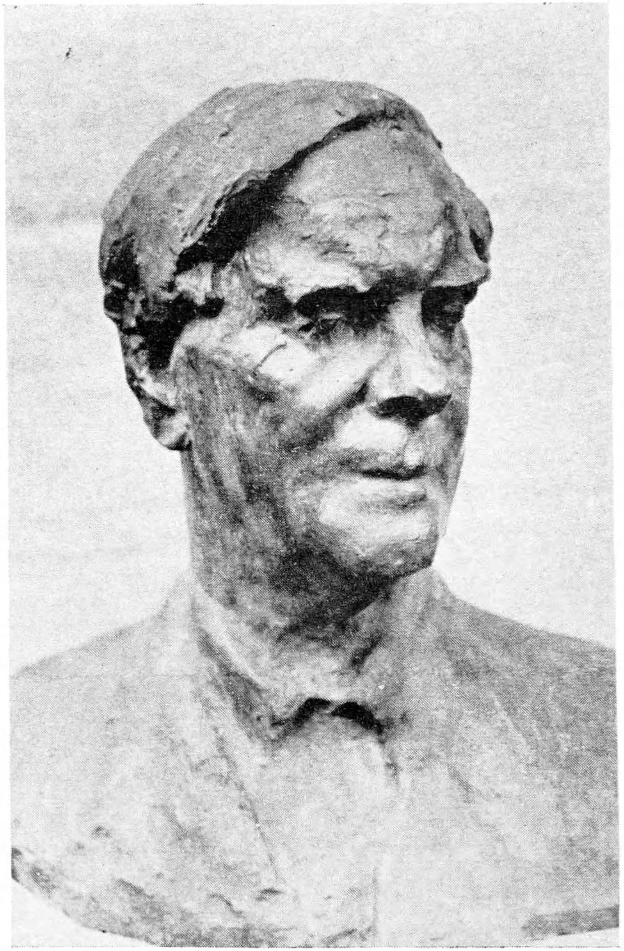

PLATE 22b.

Bust of P. L. Kapitza by N. B. Nikogosyan in the Tretyakov Gallery.

the mosaics, primarily portraits which to this day beautify the walls of the Academy of Sciences.

All this immense social-science work has been well-studied, but until recently the work of Lomonosov as a research scientist was forgotten, although it was known that Lomonosov had devoted himself to this activity with very great zeal. As a research scientist Lomonosov was an experimenter and so he occupied himself most of all with chemistry and physics. But this research appears to have been overlooked until the beginning of our present century when it first came to be studied by professor B. N. Menshutkin. From Menshutkin's historical inquiries it was discovered that Lomonosov had done immense original work in physics and chemistry which had been on the right road and had often been in advance of western science at the time. Most clearly this is seen in his complete theory of the kinetic theory of heat and in the law of the conservation of matter which was discovered by him 17 years before Lavoisier. Lomonosov's works in astronomy and also in atmospheric electricity are original and they deserve great attention too. Now undoubtedly as regards the scale and also the achievements of his scientific work Lomonosov deserved to stand in line with the greatest scientists of the time, such as Newton, Galileo, Copernicus, or Leibnitz. The history of world science shows that in actual fact he had practically no influence upon its development. It is to this contradiction that my discourse "Lomonosov and world science" is devoted.

Apart from this scientific and social-science work Lomonosov has also drawn attention to himself through his exceptionally romantic biography and the attractiveness of his personal image.

Lomonosov was born far from Moscow on the shores of the White Sea and was the son of a thriving peasant-fisherman. From childhood he plied his trade and his father regarded him as the successor to it. But at the age of 19 the youth Lomonosov was eager for learning, and against his father's will and also without means, he came on foot to Moscow from Arkhangelsk (this is 1000 kilometres, about 620 miles!) went to a Church school, lived in want and then having overcome all material difficulties along the way he finally, having finished at school, entered the Academy of Sciences which sent him to Germany to receive a higher education. On returning from Germany he was appointed professor of chemistry at the Academy of Sciences and all his work was carried on within its walls. These few facts are already sufficient to appreciate Lomonosov's purposefulness, spirit and will-power. Besides these qualities Lomonosov still distinguished himself by his immense physical strength and his boldness. For instance, it is generally known how he got the better of three bandits who attempted to rob him in the forest in the environs of Petersburg. He put two to flight, stripped the third and brought in his clothes as trophies. His spirited temperament and also the zeal with which he fought to accomplish the tasks which confronted him, often brought him into personal conflict with people and created enemies for him. Especially strongly Lomonosov fought religious prejudices which in his opinion had interfered with the country's cultural growth. He had grave conflicts with leading clerics. On the other hand the brilliance of his talents, his deep sense of honour and his dedication to science and knowledge attracted to Lomonosov the foremost people of Russia of that period.

I think that a specially vivid imagination is not required to visualise by now from this very short description, Lomonosov's remarkable image and so understand why everyone in our country considers him the founder of our scientific culture, why the image of him is so attractive and also why the memory of him is so highly esteemed.

To speak of Lomonosov is fine, just as a discussion with one of the most original geniuses in the history of human culture is fine. To speak now of Lomonosov is hard since all of us are familiar from our schooldays with his image and with his work. It is hard to add anything new since for 200 years already his life and work have been studied and discussed in all aspects. The greatest of our writers, publicists, scientists and public figures have spoken and written of Lomonosov: Radishchev, Pushkin, Belinskii, Dobrolyubov, Chernyshevskii, Hertsen, Pisarev, Aksakov, Menshutkin, Val'den, Vavilov, Fersman, Komarov and many others. Though some aspects of Lomonosov's work have indeed been criticized, yet all without exception have spoken of him with great piety and acknowledged his immense influence on the development of our national culture—our language, literature, education, engineering and science. Lomonosov's great progressive significance was recognized in the pre-revolutionary period like it is recognized also today. Ever since the last century the anniversaries of his birth and death have been constantly celebrated. In our period these celebrations are on an ever greater and greater national scale!

The first monument to Lomonosov was erected at his birthplace in Arkhangel'sk; it belongs to our greatest sculptor Martos. The subscription was started in 1825 and four years later the monument was unveiled.

In 1865 (the centenary of Lomonosov's death) the Academy of Sciences instituted an annual award in his name to the sum of 1000 roubles. The prize was awarded annually in the humanities and natural science alternately. In our period the Academy of Sciences has awarded a Lomonosov prize and medal.

The only thing that was still not done in the intervening 200 years was to publish the complete collected works of M. V. Lomonosov and this has been done in recent years.

Few of our scientists or public figures provide such rich biographical and historical material as Lomonosov; being acquainted with this material, it is a matter for regret that no good portrait of Lomonosov has come down to us. The portraits and prints which are usually reproduced were made posthumously and are copies from the same original, painted by an unknown and ungifted artist. Only the bust of the work of Shubin, who personally knew Lomonosov, gives us his living and spiritualized image.

In studying the material about Lomonosov dissatisfaction is due to the fact that none of our great writers drew his image as a human being. There are, of course, in the world many even greater scientists whose range of interests is confined to the walls of their laboratories. Usually the human form of such scientists is of little interest. But when the work of an outstanding scientist and great original man, which Lomonosov was, seizes the development of all the country's culture, and this at one of the most interesting moments in its history, his living form is of great general human interest.

The more outstanding a person, the bigger are the contradictions in that person and the bigger are the contradictions in the tasks which their life holds out. The range of these contradictions also is a measure of their genius. The contradictions in Lomonosov's very nature, as also those in which his life evolved, were exceptionally great.

It is difficult to find a greater contradiction than in the fate of the "Arkhangel'sk moujik" to live and work amongst the nobility and high officials at the pinnacle of the Court. Lomonosov was a progressive public figure, he saw the need for people's education and science, he fought against superstition and prejudice, but to do his work he had to rely on the important personages at Court. Despite his peasant origin, he understood the need to flatter and extol powerful rulers and in his own way coped with his task. By the brilliance of his personal qualities he gained the friendship and protection of the most influential personages of that time—Shuvalov, Vorontsov and Orlov.

When Peter "opened the window" onto Europe, the wind brought to us from the West not only culture and science. With the real scientists, which Euler and Bernoulli were, and who brought to us the leading western science, the wind brought to us a great many scientist-foreigners, average people, or even adventurers, interested only in material well-being and keeping up their privileged position in Russia, a country which gave them the opportunity to become rich without difficulty. It was natural that they restrained the growth of Russian influence in the Academy of Sciences. It is well known how Lomonosov, in relying on the authority of foreign scientists, had to fight against their dominance. Lomonosov by his sharp wit appraised beautifully the complexity of the conditions in which he was carrying on his activities. It required great self-restraint and tact on his part, though this contradicted the irrepressibility of his spirit and his impassioned temperament. Here arose those sharp conflicts which are well known from the biography of Lomonosov. In the last analysis in this complex struggle the genius of Lomonosov still succeeded in triumphing, though the picture of this complex struggle has not as yet been well drawn.

Lomonosov understood the great importance of developing science in Russia and the need for raising higher education; he much worked on creating in Moscow a university, he attracted young people to scientific work, but could not himself devote to scientific work as much time as he wanted. Apparently by nature he was not a teacher. His excessive individualism did not make a steadfast teacher out of him. As a result, after having put so much effort into furthering science in Russia, all the same he left no scientists behind him. Menshutkin, the greatest expert on Lomonosov's scientific work, says "that he created no school, from his pupils after his death on the scientific side came only S. Ya. Rumovskii", subsequently professor of astronomy at the Academy of Sciences.

The list of contradictions in the life of Lomonosov could be continued, but to paint the personality of Lomonosov, made up of all these contradictions, is a task which awaits its own great writer.

I would now like to dwell on one of the contradictions in Lomonosov's life which although also well known, has still to receive due clarification. I think that it is a topic of concern to us today.

More than once Lomonosov said that his work as a poet and writer, reformer of the Russian language, historian, statesman, geologist and administrator satisfied him little, and his main calling he saw in scientific work, in physics and chemistry. It would seem that scientific work in chemistry and physics must have been his main activity since from the very beginning of his stay at the Academy of Sciences from 1741 onwards he occupied the position of junior physics assistant, and four years later he was appointed professor of chemistry. It is natural to suppose that under these conditions the genius of Lomonosov ought to have left a most prominent trace in world science as well as in national science. But we know that this did not happen and this has repeatedly puzzled many who have studied the history of science. Academician P. I. Val'den in his speech given in the Academy of Sciences on the occasion of Lomonosov's bicentenary in 1911, dwells in detail on this point, pointing to the "tragedy in the fate of Lomonosov's works which have left no visible trace in chemistry and physics". Val'den cites facts which confirm that the scientific work of Lomonosov was unknown to foreign historians.

In the detailed history of physics of Heller (1889) and of Rosenberger (1882–1890) Lomonosov's name is not met with at all. The French historian of chemistry F. Hoefer (1860) writes only a few lines about him, not devoid of curiosity. I quote them in the original "Parmi les chimistes russes qui se sont fait connaître comme chimistes, nous citerons Michel Lomonosov, qu'il ne faut pas confondre avec le poète de ce nom".*

But whereas in the West they almost did not know of the scientific works of Lomonosov as physicist and chemist, even in our country they remained unknown or forgotten until most recent times. In all the vast material concerning the investigations of Lomonosov, until the beginning of the present century there are only two anniversary articles about Lomonosov as a physicist, both published in 1865; one, by N. A. Lyubimova, is a feeble rehash of several of Lomonosov's papers, the other—only five pages altogether—by N. P. Beketov. In the two big Russian encyclopaedias, Granat as well as Brockhaus, and also in the Encyclopaedia Britannica and in the French Larousse, nothing is said of Lomonosov's achievements as a physicist and chemist. Even in our Khvol'son's basic physics course which meticulously cites the literature, there was no single reference to Lomonosov until Menshutkin's work appeared.

On the other hand A. S. Pushkin in his Notes *Journey from Moscow to Petersburg* (1834), sorting out Lomonosov's activities, says "Lomonosov himself did not value his poetry and troubled rather more about his chemical experiments than about official odes to the solemn occasions of celebrities". Pushkin speaks of Lomonosov as a great figure in science; his splendid words have gone down in history "He, it is better to say, was himself our first university". Pushkin saw the genius of Lomonosov as a scientist. For us Pushkin's opinion is very important as coming from a most educated person profoundly understanding Russian reality. Moreover Pushkin could still meet persons who had seen and heard the living Lomonosov. So even by his contemporaries Lomonosov was acknowledged as a great scientist. But characteristically no-one from his milieu could in fact describe what Lomonosov in fact did do in science, why he is to be considered a great scientist.

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* "Among the Russian chemists who have become well-known chemists, we cite Mikhail Lomonosov, who is not to be confused with the poet of the same name". [Readers may accept that the poet and physicist are in fact the same person (*Translator*).]

So it continued until the beginning of the present century when Boris Nikolayevich Menshutkin, professor of physical chemistry, as a scientist started to study the original scientific works of Lomonosov in chemistry and physics. Menshutkin translated Lomonosov's papers from the Latin and German, critically studied not only the basic works, but also the correspondence and personal notes. Starting from 1904 Menshutkin systematically published this material. Later this work was continued by S. I. Vavilov, T. P. Kravets and a number of other scientists. So, only after 200 years did we find out on what Lomonosov worked and how. Now, knowing along what road science developed after Lomonosov, we can unerringly appraise his scientific work in chemistry and physics. So only now has it been discovered that for his own times Lomonosov's scientific work was the most advanced and, undoubtedly, it should have made a profound impression on the development of world science. Pushkin's intuitive assessment of Lomonosov more than a hundred years ago as a great scientist was right. All this more than ever makes us wonder how it could happen that all this scientific work of Lomonosov could pass without trace not only abroad, but also here with us? Of this one has to speak with sorrow since as a result our own and also world science suffered a considerable loss. Of course, the isolation of Lomonosov's scientific work from world science could not have been accidental, it had its historical causes. I think that the instances when the discoveries and achievements of Russian scientists have not shown their due influence on the development of world science, have not been few with us. For this reason the contradictions between Lomonosov's most outstanding achievements in science and their failure to have due influence on the development of world science are of interest also in our times. I shall dwell on this topic in more detail.

In order to do an analysis of the relation between Lomonosov's work as a scientist and the science of his day, one needs if only in most general outline, to draw a picture of the conditions under which natural science was developing in the first half of the eighteenth century. Remember that in the cultural history of mankind only the sixteenth century can be regarded as the beginning of an intensive growth of natural science. Until this time mankind also knew great scientists, like, for example, Pythagoras, Archimedes, Avicenna, but they were lone creative geniuses. Science at that time was developing slowly. Only since the sixteenth century has science started to develop at its present rate as a result of scientific work becoming a collective "creatorship" of people studying on an international scale. The first huge successes of this collective "creatorship" of scientists are well known: this was the rapid growth of astronomy and mechanics. The Pole Copernicus, the Dane Tycho Brahe, the German Kepler, the Italian Galileo, the Englishman Newton, the Frenchman Descartes, the Dutchman Huygens and still many other less known scientists.

And to this day the collective work of scientists on the international scale is the main factor ensuring the rapid growth of science. It became possible not only owing to the growth of material well-being of people and development of the means of communication between countries, but, chiefly, this became possible owing to the invention of the printing press in the fifteenth century. All the scientists well understand that to this day without books it is neither possible to disseminate, nor to preserve scientific experience, and without this, of course, science cannot develop full-valuedly. At the same time science was breaking from the Church, which was necessary for science to develop on a healthy materialistic basis.

Since then leading public figures have begun to understand the value of the development of natural science for the growth of human culture. By the beginning of the seventeenth century the immense value of experimental study of nature and the inductive method of generalization of these experiments, which leads to perception of the laws of Nature, were clearly formulated by Francis Bacon. A great public figure, who attained the position of Lord Chancellor, Bacon in 1621 was convicted of venality. He spent the end of his life in semi-banishment where he wrote the philosophical works which made his name immortal. Thus infamy in life was converted into fame after death. In Bacon's writings, in one unfinished work, which he entitled *New Atlantis,* he in a new fashion revived the history of Plato's Atlantis. This island is populated and controlled by scientists. In his description of the island one can find the scientific institutes and other sides of the organization of scientific life which resemble our state organization of science.

The value of science as a mighty force directing the growth of the country's culture along the right path is given by Bacon in the following beautiful passage where science is contrasted with empiricism "A lame cripple going along the right road can overtake a trotter if the latter is running along the wrong road. Moreover, the faster the trotter runs, once having lost the path, the further he lags behind the cripple". Also Bacon proclaimed physics as the "mother of all sciences" who first indicates the path of cultural development of mankind. I give this description in so much detail because Bacon was widely read at the time and his *New Atlantis* ran into many editions. His views spread in the ruling cliques of advanced countries and at this time the development of science came to be regarded as a State concern. It was then that scientific work so spread that the need for coordinated work arose, therefore by the seventeenth century academies of science or analogous scientific societies started to be created in many countries. Scientific periodicals and transactions began to be printed.

Peter the Great in his visit to Europe quickly perceived the value of science for development of the country and, of course, he could not help but understand that Russia had need of science to become a leading cultural country also. Then the famous discussions on this theme between Peter and Leibnitz took place and the idea of creating an Academy of Sciences in Russia arose. Our Academy was created shortly after Peter's death under Catherine I in 1725. It is well known that the Academy was formed from foreigners for them to train Russian scientists. We know that Lomonosov was lucky—he opportunely found his way to Petersburg to become one of the first Russian scientists in the Academy of Sciences. But, of course, the Academy of Sciences was still more lucky that the first Russian scientist was to be Lomonosov. He received his higher scientific education in Germany where he studied for five years, mainly under professor Christian Wolf. In 1741 Lomonosov returned to Petersburg where he had to start his scientific activities in very unfavourable conditions.

By this time the Academy of Sciences had been in existence almost 20 years, Anne was Empress, Biren reigned,* and Peter's idea of developing Russia's own science started to take second place. In creating the Academy of Sciences only two of the invited foreigners were outstandingly great scientists, both of whom had become famous. These were Leonhard Euler and Daniel Bernoulli. But due consideration for them declined and by 1741 when Lomonosov returned from Germany both of them, first Bernoulli, and then Euler, had left the Academy. It is interesting that Euler left Petersburg three days before Lomonosov returned from Germany and came back to Petersburg only under Catherine II when consideration for scientists began to improve again. But this was a year after Lomonosov's death. Thus, though Lomonosov corresponded much with Euler, they never met in person, unless one counts Lomonosov's possible attendance at Euler's lectures before his departure to Germany.

Thus in the Academy of Sciences his physics and chemistry work was left to almost total isolation. For the development of science he had to watch the published literature, which at that time was sparse, and he had no personal contact with great scientists where he was, since Lomonosov never once travelled abroad after becoming a scientist, whilst foreign scientists never came for communication with him in Petersburg, the Academy of Sciences at that time being of no interest.

Yet despite this severance from world science Lomonosov still knew how to concentrate his efforts on the most challenging problems of chemistry and physics at the time. As a scientist he combined in himself a thinker and an experimenter.

His opinions on the relation between theory and experiment are interesting, they are quite apt to this day "Some theoreticians, without any preliminary tests who abuse their leisure by inventing empty and false theories which clutter up the literature . . ."

In the study of Nature, Lomonosov regarded experiment as of paramount importance; it is this which is his characteristic feature as a scientist. He therefore put much effort into creating a laboratory and diligently worked there. But the social milieu little valued Lomonosov as a scientist, they valued him above all as a poet. For one of his odes of praise Lomonosov received 2000 roubles from the Empress, which was more than three times his salary at the Academy of Sciences (660 roubles per annum).

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* Biren, Duke of Courland, Grand Chamberlain of Russia, favourite of the Empress, is reputed to have reigned with great cruelty, was banished to Siberia, but his honours were restored later (1687–1772) (*Translator*).

Lomonosov was also valued as a historian, as the founder of the literary Russian language, for his grammar, for his translations, they valued him as a public figure who concerned himself with the development of education and engineering in Russia.

The value of his scientific studies in the laboratory was not intelligible to officials or at Court. To put himself right for his laboratory studies, Lomonosov in 1753 wrote to Count Shuvalov "I am assuming that I shall be allowed a few hours a day for physical and chemical experiments instead of billiards . . .". Lomonosov thus had to justify spending his leisure time on scientific work instead of playing billiards. Of course, the justification for the expenditure of state funds on the laboratory was the practical results, like, for instance, the production of mosaic glass and the solving of various engineering problems.

One is astonished at how much Lomonosov did in the field of experimental basic science in spite of these unfavourable conditions. Firstly, he grasped very widely in his works the various fields of physics. He studied the liquid, solid and gaseous state of bodies. He developed thermometry thoroughly and exactly calibrated his mercury thermometer. By use of them he, for instance, determined the coefficient of expansion of gases when heated with astonishing accuracy for his time. Comparing his data with ours today we find that he made less than three per cent error, which was ten times closer than the value then accepted. This shows Lomonosov's exceptionally advanced technique as an experimenter. Enumeration of Lomonosov's other achievements in experimental physics and chemistry, which were all at the same high level, would take too much time and it is not our task. Persons who are interested in this topic can read B. N. Menshutkin's excellent monograph on the works of Lomonosov in physics and chemistry, published in 1947.

Undoubtedly these papers of Lomonosov must by themselves have placed him in line with the greatest experimenters of the time. It is interesting that Lomonosov's experiments on electricity, in which he developed Franklin's papers, are better known, not because of their scientific results, but because they led to the death of Richman, killed by a lightning discharge. This research led Lomonosov to advance an interesting hypothesis concerning the nature of the electric charge in clouds.

He also carried out some optical research which boiled down to the construction of improved optical instruments, such as, for instance, the reflex telescope by which Lomonosov in 1761 observed a rare phenomenon—the passage of Venus across the sun disk. These observations were also a big contribution to science. He noted the deformation and diffusiveness of the edges of the Venus disc and it was thereby first shown that on Venus there must be an atmosphere. It is interesting that in present-day astronomical reference books it is written that the demonstration was made only in 1882, i.e. 121 years later, when Venus again passed across the sun disk.

Lomonosov's biggest achievement in its significance was his experimental proof of the "law of the conservation of matter". His discovering of the law of the conservation of matter is now well studied and it has been fully established that Lomonosov was the first to discover it. Lomonosov's experiment was analogous to the celebrated experiment of Lavoisier, though Lavoisier's was 17 years later. I shall not repeat in detail all this history, most perple know it. Doubtless this discovery of one of the most fundamental laws of nature ought in the history of science to have placed Lomonosov's name in line with the greatest world scientists.

But all these works of Lomonosov not only were not known abroad, but until the cited investigation of Menshutkin, most of them were not known in Russia either. Obviously, in these conditions the works of Lomonosov in physics and chemistry could not show due influence on the development either of world science or our science.

But why was this so?

The first reason for Lomonosov's work being little known abroad, could be, it would seem, that he did not attach importance to the priority of his discoveries and insufficiently publicized his works. No less importance was attached to priority in scientific work at that time than now. It is sufficient to recall the controversy over the priority for the invention of differential calculus between Newton and Leibnitz which turned into a major diplomatic incident; here Leibnitz's career was greatly damaged.

The materials coming down to us show that Lomonosov also attached importance to priority, so he published his papers either in Latin, or in German; he mastered both languages beautifully. The following fact serves as evidence that Lomonosov was concerned that his scientific works should be known abroad. In 1763 when Richman was killed by lightning, the general meeting of the Academy of Sciences was adjourned, but Lomonosov requested that he should be given the opportunity to deliver his speech on electricity "lest it lose novelty". For this reason the President of the Academy of Sciences Count Razumovskii enjoined a speech day to be arranged on the anniversary of Coronation Day "in order that gospodin Lomonosov should not be late with his own new productions amongst scientific people in Europa, and his paper thereby be lost in electrical experiments made meanwhile". Lomonosov's speech after this was sent to many foreign scientists. It is also known that Lomonosov wrote of his work to Euler and to a number of other scientists. It should be remembered that private correspondence was at the time regarded as one of the most effective methods of scientific information and all used it extensively. Thus there are no grounds to suppose that either abroad or in Russia scientists could not have known of Lomonosov's works. They knew them, but they did not pay due attention to them.

Some biographers of Lomonosov have put forward the suggestion that the lack of attention to Lomonosov's works was because his ideas were too advanced. I think this suggestion is unfounded also. In reality the living and bold mind of Lomonosov's grasped almost all the fields of natural science within the range of interests of the "natural philosophy" of the day. In width of scope it is difficult to name another scientist, a contemporary of Lomonosov's, with such many-sided interests and knowledge. Lomonosov's theoretical conceptions in those fields of science where he directly did his experimental works—study of heat, the state of matter, chemistry—are startling in that they coincided in detail with the path along which these fields developed after Lomonosov and are still developing today. Very surprising for modern readers is the fact that the kinetic nature of heat was quite clear to Lomonosov. He related the heating of a body to increasing translational and circular motion (rotational motion) of atoms and molecules, which of course he named differently. In physics at the time the false representation of the existence of a "thermogen" was dominant. Although these views of Lomonosov's were ahead of his time, yet he was not the only adherent, for instance, they were shared by Bernoulli. In developing these views Lomonosov was extremely consistent and logical, for instance, he came close to the concept of absolute zero. In his reflections *On heat and cold* in Section 26 he speaks "of the highest possible degree of cold caused by the total rest of the particles, the discontinuance of any motion thereof".

The following curious fact may serve to illustrate Lomonosov's firm belief in the correctness of his own representation of the physical essence of heat. In 1761 Lomonosov wrote the note *On the multiplication and preservation of the Russian people.* In this note he considered the various factors which caused the heavy mortality in Russia and proposed a number of counter-measures. Thus, in Section 7, he writes that children always need to be christened in warm water "priests implement the prescriptions of the Prayer-book that the water should be natural without admixtures and impute heat to be an admixed material, but they do not think that in Summer they themselves christen with warm water, according to their opinion mixed, and so contradict themselves; whilst, more particularly, in their thoughtlessness they do not know that even in the coldest water there is still very much heat. But no need to interpret physics to know-nothing priests".

It is interesting that this note was never published in Tsarist times since the thoughts expressed in it were too revolutionary.

The ideas of Lomonosov which guided his work in the field of chemistry were also advanced and perfectly correct. He always proceeded from the atomistic representation and came close to the notion of the molecular structure of chemical compounds. In scientific investigations in chemistry he considered it necessary to use the quantitative method and considered it important to use as far as possible pure reagents. This quantitative approach to the study of chemical reactions thus also led him to the necessity for experimental proof of the law of the conservation of matter. All this with good reason permits one to regard Lomonosov as the pioneer of physical methods of investigation in chemistry, as understood in the eighteenth century.

In the field of electricity Lomonosov worked less. The experiments of Franklin, his contemporary, were known to him and he repeated them, though Lomonosov's main interest was in topics associated with atmospheric electricity. He linked its origin with the ascending and descending streams of air which always accompany thunder clouds. This opinion also is considered to be right to this day, though the mechanism itself of the charging of clouds appears so intractable that it has still not been finally established.

In the field of wave optics Lomonosov together with Euler correctly supported the wave theory of light proposed by Huygens, an obstacle to the recognition of which was the authority of Newton, who had stubbornly persisted in his erroneous corpuscular theory of light. But in the further development of the theory of light Lomonosov took a wrong path. The same also happened with Euler.

The biggest error of Lomonosov's in one of the fundamental problems of physics is of great interest.

As is generally known Galileo discovered one of the most remarkable laws of Nature. He established that the mass of a body, of whatever nature, is proportional to the force of gravitation, or at a particular point in space simply to its weight. Newton showed that this law is satisfied with great accuracy. His experiment was very simple, precise and convincing. Alone by himself in his room at college, Newton suspended in the doorway two pendulums of identical length, but made from different substances. It appeared that the pendulums always oscillated strictly isochronously independently of the suspended substance. This could only occur if the mass of the body were exactly proportional to its weight. Lomonosov considered that this was incorrect. He began to speak out on this theme in 1748 and continued to do so until 1757. All these utterances came much later than Newton's experiment with the pendulum. But Lomonosov continually fought this law surprisingly stubbornly. Thus in 1755 he proposed, as the problem for the prize-winning award of the Academy of Sciences, experimental verification "of the hypothesis that the matter of bodies is proportional to weight". The posing of this problem, as contradictory to the views of the great Newton, met with objections in the Academy of Sciences and Euler was brought in as judge. Euler, who was usually on Lomonosov's side, did not in this case support him and was against posing such a problem. It should be mentioned that Lomonosov's only pupil S. Ya. Rumovskii also did not share Lomonosov's opinions, as is clear from his letters to Euler in 1757. Rumovskii, who later became an academician, had studied mathematics for two years under Euler in Berlin and, naturally, he knew Newton's mechanics well. It is possible that at the time Rumovskii succeeded in showing to Lomonosov his blunder since after 1757 I have found nothing to indicate that Lomonosov again raised this question.

Nothing is so instructive as the floundering of a genius. It seems to me that in this particular case this floundering was not accidental, but had a more profound cause. To gain a really satisfying understanding of this question, I should have to pay rather more time to it than I can.

I suppose the cause of Lomonosov's floundering is linked with a philosophical concept to which he mistakenly imparted universal significance. This concept of Lomonosov's consisted in that motion in nature is always conserved, never emerging and never lost, but just transmitted from one body to another and then only by direct contact. We know that such a representation holds good for elastic impact of spheres. Now we also well know that, by regarding collisions between atoms and molecules as collisions between elastic spheres, one can construct a complete and correct picture of the kinetic nature of heat. It is therefore understandable why Lomonosov, taking on the one hand, the atomistic structure of matter, and on the other subjecting interaction between atoms to the laws of the collision of elastic bodies, first was able correctly to construct almost a complete picture of thermal phenomena on the basis of the kinetic conception. As I have already said, he not only got down to defining absolute zero, but also got right down to formulating the law of the conservation of energy, of course, not in general form, but just for the conversion of kinetic energy into thermal energy.

The error of Lomonosov was that he imparted universality to his concept and began to consider that in Nature only a single means of interaction between bodies exists and this by osculation. He denied the possibility of action at a distance by gravitation or electric interaction. Developing such ideas he considered that if a body gains speed under the influence of gravity, the medium surrounding the body must necessarily lose speed. A medium possessing the property to generate motion was, of course, hypothetical and its existence in Nature Lomonosov postulated. In a similar way he attempted to describe also electric interaction between bodies. It is not hard to see why on a foundation of such representations Lomonosov not only did not succeed in drawing a clear picture of the phenomena associated with the interaction of bodies at a distance, but this led him to deny the existence of a universal relationship between the weight and mass of bodies.

It is hard to understand how Lomonosov, in developing these views, could not reconcile himself with Newton's experiments with the pendulum. Possibly he either did not know of them, or did not understand them; I have been unable to find any mention of these experiments anywhere in Lomonosov. Upon acquaintance with Wolf's physics course which Lomonosov took and which he translated into the Russian language, it is striking that due attention is not paid to Newton's works in mechanics. Of his experiments with the pendulum also there is no mention. It is interesting that the only question of mechanics to which Wolf paid attention was precisely this colliding of spheres. I compared the writings of Christian Wolf with those of other physicists of that time; he produced on me the impression of a scientist with limited physical thought. It is generally known that his fame is owed to works on abstract philosophic themes. Apparently Wolf did not instil in Lomonosov elements of concrete mathematical thought, without which it is difficult to grasp Newton's mechanics. As I have pointed out, Lomonosov did not have the opportunity to meet such scientists as Euler and Bernoulli, who not only knew Newton's mechanics beautifully, but also themselves became famous in that they developed it for a continuum. One can say with certainty that if this social intercourse had existed, this floundering of Lomonosov would not have happened.

The saddest thing about Lomonosov's fate was that he could only devote a small fraction of his energy and time to his experimental work. But with his great erudition and exceptional imagination he did not have the opportunity to subject to experimental verification all the hypotheses which he advanced. Therefore so it also came about that in those fields where Lomonosov worked experimentally, his theoretical and philosophical representations lay on the right road. But wherever he was divorced from practice and where he attempted to comprehend the truth by the deductive way, he was often led astray. If he had been placed in conditions where he could have more widely extended his experimental work, for instance, if he had had many pupils, then most likely, there would have been far fewer erroneous hypotheses. With his exceptional imagination Lomonosov could have been the leader of a great scientific school. But in Russia at that time conditions did not exist for creating such a school.

Thus the explanation that Lomonosov was not recognized as a scientist because he was far divorced from reality, has no foundation.

Here it is pertinent to recall that, generally, in the history of Russian science, Russian scientists have often been isolated from world science. It seems to me that one has to look for a general cause which lies deeper than those cited. But before passing on to its examination I think it is useful briefly to recall another unrecognized Russian discovery, which is extremely reminiscent of the case with Lomonosov.

At the very beginning of the nineteenth century in our country a very great discovery was made in physics which also did not have its due influence on world science. It happened in 1802 when Vasilii Vladimirovich Petrov discovered the phenomenon of electric arc discharge in gas, called by him the "voltaic arc". We today all know well the immense part which arc discharge has since played in engineering as well as in science. But now, in our times, it is hard to assess on merit all the difficulties in discovering this fundamental phenomenon, first done by Petrov. The discovery was made within eleven years of the discovery of galvanic current and within only three years of Volta's galvanic column. Of course, during these three years little of galvanic current was generally known. Petrov not only had to make for himself the batteries which consisted of 4200 copper and zinc discs, stacked in a column more than three metres in length, but he himself also had to make the wire, insulating it with rubber.

Petrov not only observed the arc column at normal pressure, but also at high pressure, passing current in the bell of a vacuum pump. The discovering of such a type of discharge can today be regarded as the discovery by him of a plasma. Although Petrov's works were published as a separate pamphlet and circulated to many scientific institutions of the time, all the same the discovery of arc discharge is usually ascribed to Davy, though he made it only in 1810. Many other interesting works in luminescence and chemistry belong to Petrov. He, apparently, was the first to produce dissociation of water by electrolysis, but none of these works had due influence on world science either.

Petrov's biography is very instructive. The son of a local priest, he began his carreer as a humble tutor in Barnaul in a provincial college, later attaining the position of professor of physics at the Medico-surgical Academy in Petersburg. Petrov, as also Lomonosov, was a lone scientist and he also left no school after him. His works and he himself remained unmentioned in the history of science not only abroad, but also in Russia. No portrait of Petrov has been preserved, and only recently has it become known where he was buried. For me there is no doubt at all that for his scientific discoveries Vasilii Vladimirovich Petrov ought to have occupied one of the leading places not only in our science, but also in world science as a great experimental physicist.

One often hears that the lack of attention to the achievements of Russian scientists is explained by the fact that Slav culture has usually been regarded as playing a minor role in the West, not worth taking into account in the history of world culture. Undoubtedly, in the eighteenth and nineteenth centuries this attitude to Slavs in general, and to Russians in particular, was fairly common, but I think that it cannot be the answer since the history of science shows that the appreciation of great scientists' scientific achievements has always overridden national frontiers. Didn't they recognize Copernicus, though he was Slav. Enough also to remember how more than once Euler spoke highly of Lomonosov's works. Furthermore this does not explain why we ourselves so underestimated the scientific work of Lomonosov, Petrov and a number of other Russian scientists.

It seems to me that the explanation needs to be sought in the conditions under which science develops in a country. It is not enough for a scientist to make a scientific discovery to show his influence on the development of world culture,—it is necessary for definite conditions to exist in a country and for the necessary link to exist with the scientific community abroad. Unless the conditions are present, even such remarkable scientific works as those done by Lomonosov and Petrov, cannot show influence on the development of world culture.

These conditions which were necessary in Lomonosov's time, the same as they are important in our times, are what I want to dwell on.

As I have already said, since the seventeenth century owing to the collaboration of scientists on the international scale, the natural sciences have been developing much more quickly than formerly. This has been possible only because these sciences, when they are developing on an experimental basis, are a single whole for all humanity. This property of unity of materialistic science has also made possible its development in the wide international fraternity of scientists. The system by which international cooperation of scientists takes place is well known; it is the same today as it was in Lomonosov's time. The various countries all have their own groups of scientific workers at universities, academies or other scientific institutes. Since each scientific field or problem can only be developed in one way, so as not to stray from this true path, one has to move slowly with much wasted effort on research. Collaboration in scientific work consists also in distributing this laborious research between the collectives of scientists working on a particular question. The work of a scientist carrying on outside a collective usually remains unnoticed.

Life is continually showing that this collective work of scientists within a country, and also in the international scale, is possible only with personal contact. For a scientist's work to be recognized, it is necessary not only to publish it, he still has to convince people of its validity and show its value. All this can only be done successfully with personal contact. As in Lomonosov's time, so also today, for a scientist to be able to influence the collective work by his own work, social intercourse is necessary, a lively exchange of opinions is necessary, discussion is necessary, neither publication nor correspondence can replace all this. Why this happens is not so easy to explain. I think that most of us know from our own experience how necessary it is to have personal contact between people for harmonizing creative activity. Only when you see the man, see his laboratory, hear the intonation of his voice, see the expression on his face, does confidence in his work and the desire to collaborate with him manifest itself. For this same reason no textbook can replace a teacher.

Today the necessity of personal contact between scientists is taken as a matter of course both by our and also by foreign scientists. Contacts are growing all the time and now usually they are made on a large scale by congresses and conferences.

In Lomonosov's time private meetings of scientists were already widely developed. Usually this is what happened. In a given field of knowledge in any country a leading centre of scientific work was formed. Naturally such a centre attracted other scientists, often working on their own. In the eighteenth century the most powerful science was in Great Britain. This is explained by the exceptional wealth of the country for that time, the Mycenae of which backed science, and it could more widely develop. There, for instance, from America came Franklin who, like Lomonosov, was a lone scientist. He gained recognition for his outstanding works on electricity when he announced them at the London Royal Society. Also after going to London, Leeuwenhoek obtained full recognition for his work on the microscope, which at first had been treated with distrust.

The tragedy of isolation from world science of the works of Lomonosov, Petrov and other lone scientists of ours used only to be that they could not join in the collective work of scientists abroad since they did not have the opportunity to travel abroad. And it is this which answers the question why their works had no influence on world science.

Now it still remains for us to dwell on the question why in our own country Lomonosov's scientific work so long did not obtain recognition. Quite clearly, for recognition of a scientist it is necessary that his social milieu should be on such a level as to understand his work and appraise it in essentials. Naturally, neither the administrative bureaucracy, nor the nobility, surrounding Lomonosov, could understand the value of his scientific work, and therefore recognition of his work in physics and chemistry only then became possible when in our own country a scientific community of our own appeared.

It is necessary to dwell on this lesson of history so as to appreciate the immense part which the community plays in the organization of science. Today this is very important to us since we are faced with the task of creating the leading science.

It is well known that a link with the community is necessary for successful development of any creative work. Writers, actors, musicians, artists cultivate and develop their talent to the full only if it is linked with the community. The scientist's creativity also cannot successfully develop outside the collective. Moreover, just as the level of art in a country is determined by the tastes and culture of the community, so also the level of science is determined by the degree of development of the scientific community. The tragedy of Lomonosov was aggravated still further in that, as I have already said, in our country there was then no scientific community of our own. Without a healthy critical collective it was difficult for Lomonosov to find the opportunity to see where he was on the right road in his enquiries and where he was in error.

Therefore Lomonosov could not manifest the full force of his genius. He painfully endured the lack of understanding and recognition of his works in his own country as well as abroad. He did not obtain the full joy from his creativity to which he was entitled by virtue of his genius.

It is not hard to see that development of a leading science requires the leading scientific community. Unless we create our own leading scientific community, no matter how many Lomonosovs are born amongst us, we could not create in our country a leading science. The creating of a healthy leading scientific community—this is the great task to which we are still not paying enough attention. It is harder than training selected clever young people for scientific work or building large institutes. The creating of a healthy community involves educating the wide sections of people associated with scientific work. It is necessary to school them to interest themselves widely in science, to respect and love their science, to know how to evaluate objectively the achievements of our science and back up all that in fact is great and best in science. You see, only a scientific community can help a scientist to go along the right road.

Only an advanced scientific community can appreciate the cognitional power of a scientific achievement as apart from its immediate practical value.

All the natural sciences can develop in the right direction only if they are backed by a healthy scientific community. As I have already pointed out, we discovered and recognized Lomonosov at the beginning of the present century not by accident, but only because in our country at this time a healthy scientific community of our own was beginning to grow. The fraternity in physics grew up in our country when the material conditions for scientific work were improved and when the opportunity arose for our great scientists—Lebedev, Rozhdestvenskii, Lazarev, Joffe—to create schools of their own.

Today, under socialism, when science is laid at the base of the development of the State, the influence and value of our scientific community is rapidly growing. It is necessary to remember that for our science to be the most advanced, our scientific community also must be the most advanced. It must be driving and authoritative so that its censures and appraisals are recognized on the world scale.

25. THEORY, EXPERIMENT, PRACTICE

INTENSIFICATION of the role of science in the construction of a communist society requires expansion of purposive investigations to open up new prospects in the development of productive forces, wide and rapid adoption of the latest achievements in science and engineering, and a decisive rise in experimental research.

The rise in experimental research, as defined in the new Programme of the Communist Party of the Soviet Union, is one of the most urgent problems in our science. The point is that at present there is no correspondence in the developing of theoretical and experimental work, and the links between theory and practice are becoming weaker.

Here I have in mind, firstly, all of physico-mathematical science. It is not hard to see that experimental physics is lagging behind theoretical physics. Take, for instance, all the works presented this year for the award of Lenin prizes in theoretical physics, but there is not one in experimental physics. I am the editor of the *Journal of Experimental and Theoretical Physics*, and it is well known to me that most of the articles which are sent to us relate to theoretical physics. The ratio is roughly 1 to 4 or 1 to 3. It is significant that young people on leaving college tend to go for theoretical work, and not into experimental research.

Experimental physics must not be allowed to lag behind, for this would greatly hold back the normal growth of our physics and would obstruct its gateway to advanced positions in world science over the entire front of most important research. The gap between theory and experiment, between theory and practice, is the symptom of a grave disorder in the normal development of science. That there should be a gap between theoretical physics and practice on the one hand, whilst the quality of experimental research is insufficiently high on the other, upsets the harmonious development of our science and, it seems to me, this applies not only to physics, but also to many other fields of natural science.

We have to implement the necessary measures to raise our experimental physics to a new high stage. Of course, for this it is first necessary to discover what is holding back experimental research and upsetting the normal relationship between theoretical science and practice.

From the history of physics it is well known that the division of physicists into theoreticians and experimenters occurred comparatively recently. Previously, not only Newton and Huygens, but also such theoreticians as Maxwell, usually verified their theoretical deductions and constructions experimentally

П. Капица, Теория, эксперимент, практика, *Экономическая газета*, **13**, 26. iii. 1962, стр. 10.

themselves. Now, however, only in exceptional cases do theoreticians attempt to check their theories themselves.

This is happening for the very simple reason that experimental techniques are becoming much more complicated. It requires great effort to carry out tests, which is not usually within the capacity of one person, and it is done by a collective of scientific workers. In actual fact such equipment as accelerators, liquefiers, elaborate electronic circuits, reactors etc., require a large staff of scientific workers to conduct the experiments. Therefore a theoretical physicist cannot possibly check his own theoretical conclusions in practice and he is obliged to rely on the favours of experimenters and to wait until they decide to verify his conclusions and proposals in practice.

Thus non-correspondence arises between the amount of theoretical work and the possibility of subjecting it to experimental investigation. A theoretician may well produce several papers per year, say four, but to do the experimental checking, a year or eighteen months is usually required and a group of, shall we say, five persons has to work on each. Clearly the theoretician will have to have between 20 and 30 experimenters. This, of course, is a simplified roster, but in general it shows the correlation between theoreticians and experimenters necessary for scientific development.

At present the number of theoreticians and experimenters is approximately equal, and the result is that the majority of theoretical conclusions are not being verified in practice. Theoreticians lose the habit of thinking that none of their work acquires a valuation until after experimental verification. The theory begins to work of its own accord, and in the best case the value placed on it depends upon epistemological and aesthetic considerations.

For harmonious development of science, of course, it is necessary for theory not to lose touch with experimentation, and this can only be the case when theory rests on a sufficiently large experimental basis.

Why do so few people with us go into experimental work and why is it with us inadequately organized? The answer to this question is simple: in our times the work of the experimenter is rather more arduous and less "profitable", and not just because the experimenter in the event of failure loses, not two to three months like the theoretician, but a year or eighteen months, i.e. the time which at present is usually necessary to complete an experiment. The work of the experimenter requires rather greater effort; not only does he need to understand the theory, but also to have a number of practical skills in handling instruments, and he needs to create a well harmonized team. Often the experiment requires continuous work day and night. All this leads to the fact that recognition of an experimenter as a scientist who has achieved scientific status, comes much later than for the theoretical physicist.

To submit and defend a thesis from work done collectively, he has to separate the part which is supposedly his independent contribution, and this must be confirmed by his director. It is easy to see that this requirement is radically at variance with that healthy spirit of collective work when persons are continually exchanging experience and ideas, helping each other and "swopping over". The separation of "personal ownership" for defence of a thesis is an unnatural factor holding back the development of collective creativeness. This repels many people, especially young people, from experimental work.

The director of an experimental group is also placed in a difficult position. He bears the responsibility for the work, but since often he himself takes no part in many of the divisions of work, it is usually considered that his name should not appear in the list of authors. Young people underestimate the director's role, though he selects the team, divides the duties between its members, and separates the good ideas from the bad. Naturally, the director's function is extremely important. In modern conditions the director of scientific work is like a stage producer—he creates the show, but he himself does not appear on stage.

At present it is very difficult to attract capable scientific workers into supervisory posts—heads of laboratories, directors of institutes. These positions are often filled by personnel with administrative skills, without the creative scientific qualification. And this lowers the quality of the scientific experiments about which we have been speaking.

In such large fields of experimental physics as space research, plasma-physics, nuclear-physics, design of accelerators, scientific teams are very big, and the role of the director is decisive. Only the right appointment can ensure success.

Let us take other examples. They also will show that the experimenter is in more unfavourable conditions than the theoretician. For instance, if a theoretician is awarded a prize, he alone receives it. When, however, a prize is received by a large team of experimenters, they each only have a very small share in it. When a theoretician does his own work, his only apparatus is pencil and paper, and even this is unnecessary to some. Thus, when Euler was blind, he did his work in his head.

But the experimenter needs a good material base, room for every possible kind of equipment, a large selection of instruments, special materials, workshops, a well-trained staff of laboratory assistants and so on. Progress and success in the work is conditional upon the perfecting of this material base. Ten years ago our material base was very poor, since then it has considerably improved, but all the same its condition is still holding back experimental scientific work, and making it less attractive for the scientist.

From all this it becomes understandable why our young people are tending towards theoretical scientific work and why such non-correspondence has grown up between theory and experiment. And this leads to theory being divorced from practice.

It is necessary to give the experimenter and director of experimental research working conditions which are at least as attractive as for theoreticians. It is also necessary to go on improving our organizational system of scientific work and, more particularly, to encourage its collective forms. Such incentives may be based on material and moral factors. For instance, one could arrange for thematic prizes for experimental work, easier methods of obtaining degrees on the basis of one and the same collective thesis.

What has been said of physics, may also apply to other fields of natural science. The separation of theory from experimental work, experience and practice above all harms theory itself. I would like to show this by an example of the work of those philosophers who are occupied with the philosophical problems of natural science.

There exists a field of science which goes under the generally accepted name—cybernetics. The many who know what cybernetics is, know also of its immense role in modern society. And here is what was written about cybernetics on page 236 of the *Philosophical Dictionary* (1954 edition): "Cybernetics (from the Greek word denoting steering, controlling) is a reactionary pseudo-science arising in the U.S.A. after World War II and which has received wide dissemination in other capitalist countries: a form of modern mechanisticism".

True, this was written in a book published eight years ago. The error has since been corrected. But, after all, philosophers should foresee the development of natural science, and not fix on a particular phase.

If our scientists then, in 1951, had listened to the philosophers and accepted this definition as a guide for the development of his science, one may say that the conquest of space, of which we are all justly proud and for which all the world respects us, could not have been carried out since it is impossible to guide space vehicles without cybernetic machines.

Here is another example which shows the consequences of an insufficient understanding and knowledge of physical experimentation. It is still fresh in the minds of many how some of our philosophers, dogmatically applying the dialectical method, proved the untenability of the theory of relativity. Most criticism was centred on the conclusion that energy is equivalent to mass multiplied by the speed of light squared ($E = mc^2$). Physicists had already subjected this law of Einstein to tests with elementary particles. To understand these tests required a profound knowledge of modern physics, which some philosophers did not have. And here the physicists produced nuclear reactions and verified Einstein's law, not on individual atoms, but on the scale of the atom bomb. A fine thing it would be if physicists were to follow the conclusions of some of the philosophers and stopped working on the problem of applying the theory of relativity in nuclear physics! In what a position physicists would place the country if they were not prepared for the practical use of the achievements of nuclear physics?

These examples most vividly characterize the chasm between some philosophers and practice, though there are still other instances, for example, the incorrect evaluation of the principle of uncertainty in quantum theory, their incorrect evaluation of the theory of resonance in the study of chemical bonds. It seems to me that such incorrect generalizations have been made by our philosophers not only in the field of physics, but also in biology.

This speaks for the use of dialectics in natural science requiring an exceptionally deep knowledge of experimental facts and their theoretical generalization. Without this, dialectics is unable to give the solution of problems. It is, as it were, a Stradivarius violin, the most perfected of violins, but to play on it, one needs to be a musician and know music. Without this, it will play out of tune the same as an ordinary violin.

The work of scientific naturalists who are making a great contribution to modern natural science, and whom Lenin called "primordial materialists", has unfailingly progressed in the close tie of theory and experience. Therefore for the developing of natural sciences on a sound materialistic basis, any theoretical generalizations must unfailingly be verified by experiment. Harmonious development of theory and practice is absolutely necessary in all fields of natural science.

Concerning the "mechanism" of linking theory and practice, I would like to recall an illustration given long ago by Kelvin. He compared theory, primarily, of course, mathematical theory, with millstones, and the test data with the grain which is poured into these mills. The product—flour—is a benefit to people. Quite clearly, some mills, however much they grind, cannot produce anything beneficial—this is theory operating of its own accord. The quality of the flour is determined by the quality of the grain, and rotten grain cannot give edible flour. Therefore good quality data are required in an experiment, both for constructing an advanced theory, as also for obtaining practical results.

High quality of experiment is a necessary condition for a healthy development of science.

26. THE FUTURE OF SCIENCE

THEY have asked me to speak on the future development of science. This is a difficult task, but one which is interesting and useful: only by having a clear perspective of the future can we rightly direct our work in the present.

I would like to remark that I am one of the oldest present at the conference. Throughout my forty years of scientific work I have had the opportunity to observe the numerous changes which have taken place in the development of science and in its tasks. Glancing in one's mind over this phase of science, it is difficult not to notice the radical changes now in the relations to it. In the days of my youth they often talked about "pure science", "science for the sake of science". Of this there is no more talk now. Science has now come to be regarded as a necessary component in the social system, and not just as useful, but as an integral part. The State is paying more and more attention to science as a most important element of State life; now scientific institutions are being placed on a par with other branches of the social system, for instance, education, transport and the army. This was not the case fifty years ago; then chance factors ruled in the organization of science, and its development was based on private initiative.

Today with the expanding of scientific research in all countries on a larger scale, State financial appropriations to the development of science continue to increase in academic and also industrial scientific institutions. Most complex plants of immense size are being constructed, like accelerators for many millions of electron volts, or large atomic reactors, artificial satellites are being launched for investigating outer space and so forth. The solution of such problems cannot be the affair of individuals, but is the result of collective creativity, requiring great organizational effort and resources which are only within the capabilities of large States.

The growing scale of scientific work is involving the dividing of science into basic (cognitive) and applied science. I think that this division in the main must be considered artificial for it is difficult to mark the point where basic science ends and applied science begins. The division is bound up with the particular aims being pursued by the scientist, whether cognitive or applied. Therefore basic science is increasingly being concentrated in academic institutes and in universities and applied science in industry. This separation of science is mostly connected with the necessity of finance, planning and supervision of scientific work.

П. Л. Капица, Будущее Науки, *Наука и Жизнь* 3, 18 (1962).

Verbatim report of a speech in 1959 at the International Symposium in Prague on the planning of science, adapted by the author for the journal *Science and Life.*

In the work of many great scientists it is difficult to trace when they were pursuing the applied aim, and when the aim of cognition. For example, the great scientist Langmuir worked all his life in industrial undertakings and he solved some of the biggest technical problems in the electric lamp industry, but it is well known that in the course of this research he did many fundamental investigations in electronics and vacuum physics.

COLLECTIVE WORK IN SCIENCE AND THE DIRECTOR'S FUNCTION

Now we are often discussing how scientific work should be organized on the scale which requires a large, coordinated creative collective. Who ought to direct the work of such a collective—a scientist or an administrator?

Professor Bernal in his address said that administrators play the decisive role and that they are necessary for organized work on big scientific problems. I do not agree with Professor Bernal—not in that such organizers are not necessary for collective work in science, this is right, but, in my opinion, because they should be scientists directly themselves and not administrators. I am best able to express my thoughts by drawing a comparison with other creative fields, namely, with the theatre and cinema.

Once the theatre consisted only of troops of actors, and the producer was an inconspicuous figure. But especially since the development of the cinema, in which thousands and hundreds of thousands of actors take part, the one who determines the success of the film is now the producer. With large-scale collective research the producer has now become necessary in science as well. What requirements do we expect of him? The main requirement is that he is creative, and not just playing an administrative role. He must understand the meaning and purpose of solutions of scientific work and he must rightly assess the creative possibilities of his executives, allocate functions according to talent, and deploy his forces so expeditiously that all aspects of the problem to be solved develop harmoniously.

Since to organize the solving of any new scientific problem one has to find the organizational forms oneself, the director of a big scientific project, even if he himself personally does not work in science, must be a person with great creative talent. I do not know why the director of such a magnificent achievement in science as the launching of the first artificial satellite is not worthy of the Nobel prize, though, maybe, he personally did not do the scientific work associated with the preparations for this unique experiment? Did he not organize it? Such cinema producers as Sergei Eisenstein, or René Clair, of whom one may say that they are great creative directors, and who, as is well known to us, created the most remarkable feature films. were they themselves not actors in this respect? We know cases when a great actor was also a great producer, for instance, Charlie Chaplin. So also in science cases are known when a great scientist is also a great organizer of collective scientific work. Such versatile scientists were, for instance, Rutherford and Fermi. But this, naturally, is a felicitous exception, and not the rule. Doubtless a period of scientific development is now setting in when the functions of the organizer are becoming greater and greater.

It seems to me that we must now begin specially to educate and train persons to be organizers of large scientific projects, and in order to make this duty attractive, to treat such persons with great respect, and not to regard them simply as some variety of bureaucrat-administrator. In a short paper it is difficult to formulate the attributes necessary to select these people and how to teach them and educate them. They are met with very rarely and, apparently, this is a unique kind of human talent, and so they need very great attention and great care.

Hence I suppose that one of the problems of the future is this educating and developing of a new type of science-organizer, with the duties and status I have just described. This type of science-manager is at present in the initial phase of development, but he will play a decisive part in the large-scale science of the future.

SCALE OF SCIENTIFIC WORK

The next problem on which I want to dwell is the scale of scientific work in the future. Shall we try to look into the question of the volume of resources (personnel, material and finance) which the State will eventually earmark for scientific work? At the present time even in the most advanced states this volume amounts to a small fraction of one per cent of the total budget. But each year this fraction is increasing in absolute magnitude both in socialist countries, and in capitalist countries.

Many economists are now recording great social phenomena associated with the rise in technical culture. And in agriculture, as well as in industry, the part played by physical effort is continually decreasing. Already, on more than one occasion, it has been pointed out in the press that with the growth of electric power supplies and with the introduction of mechanization, and automation, production will use up little of a person's strength; owing to power station energy this work will be done by cybernetic machines, whilst the released creative forces and spiritual energy of people will mainly be directed to science and the arts. It is asked, but what section of mankind will eventually be engaged in science and the arts? Here we can resort to an analogy in the Herbert Spencer style. If one compares the State organism with an animal and compare the weight of that part of the animal's body which does mental work, namely its head, with the weight of all its other parts doing physical work, we get an interesting result. Let us begin with antediluvian animals, for instance, the dinosaurs. These were reptiles with a small head and a gigantic body. We know that in the evolution of life on the Earth the future did not belong to such an animal. The future in the struggle for existence belonged to man, whose head is roughly 5–10 per cent of the weight of his body.

So also in the evolution of human society, culture will continually grow and more and more resources will be expended on it. Thus one half of the population of the State will perform the social functions, whilst the other half will work in institutes, design bureaus and at experimental plants where mechanization and automation does not apply, but an individual approach is necessary for each new problem which arises. Professor Bernal points a different picture of the creative scientific work of people in the future. He supposes that everyone will allocate one part of their time to mental creative work, and the other part to productive work. To me, personally, Semenov's suggestion seems more probable, since people who are inclined to creative work will give themselves to it wholeheartedly. This gives the persons more satisfaction, and this makes their creative work more productive.

SCIENTIFIC DISCOVERIES OF THE FUTURE

Now I want to dwell on the fields of science which, one may suppose, will arise in the future. In this respect predictions can be made from different premises. I propose to do it on the principle of extrapolation, and so I shall begin with an estimate of the number of new natural phenomena which have already been discovered. I want to qualify the expression "new phenomenon"; I apply it to such physical phenomena as cannot possibly be predicted or explained on the basis of theoretical concepts already in existence, and therefore the discovery opens up new fields of investigation. So as to make the proposed extrapolation clearer, I shall call fundamental *new phenomena* in physics *basic phenomena* if they have been discovered in the last 150 years.

First of all I want to cite Galvani's discovery in 1789 of the electric current which, of course, in no way followed from the hitherto existing theoretical concepts of the nature of electricity, essentially due to Franklin.

The next discovery coming within our definition is Oersted's discovery in 1820 of the influence of an electric current on a magnetic needle. From our viewpoint Faraday's later discovery of magnetic induction was not new since magnetic induction is essentially the obverse of Oersted's discovery, and thus at the time it could have been predicted. The work of Oersted and Faraday led to Lenz's law, to the formulation of Maxwell's equations and to many other fundamental conclusions, although they were all an elaboration of the basic discovery of Oersted, which could have possibly been predicted theoretically.

The next instance of a new phenomenon was photoelectric emission, discovered in 1887 by Hertz (we all, of course, honour Hertz rather more for discovering electromagnetic waves). This phenomenon also could not have possibly been predicted theoretically. On the basis of studies of photoemission Einstein thirty years later derived his famous equations, having determined the quantum nature of these phenomena. The principle of uncertainty and quantum theory were pre-determined by the discovery of photoemission, and all the remarkable scientific elaborations of this phenomenon only constituted further methodological work.

Next one may cite Becquerel's discovery of radioactivity in 1896, which also could not have possibly been predicted on the theories then existing, and which laid the foundations of nuclear physics. Then Thomson's discovery of the electron can be regarded as a new phenomenon, laying the basis of modern electronics. Also the experiment of Michelson and Morley, since it produced a result which it was impossible to have predicted theoretically, and one may say that the discovery of the phenomena which established the basic principles of the theory of relativity was also new. It was impossible to have foreseen Hess' discovery of cosmic rays in 1919. I daresay one also needs to mention uranium fission by Meitner and Hahn as a new phenomenon.

What is typical of all these discoveries? Above all their value was only completely recognized after 20–30 years, when it had become clear that they could not be explained by the scientific views of the times, and therefore under their influence new directions arose and developed in the fundamental theoretical conceptions.

Are such discoveries possible in the future? Have all the physical discoveries in Nature already been exhausted? Are there other fundamental phenomena in Nature which are awaiting to be discovered?

If we plot a curve marking off time on the horizontal axis and the number of discoveries along the vertical axis, and if we honestly consider this discovery curve, it is seen to have a tendency to fall to zero. Therefore, by extrapolating this curve, we see that in the near future we shall be witnesses of no more important and "new" discoveries other than those just cited. These will enable us to understand Nature more deeply and place at Man's disposal new possibilities for cultural growth. Usually people are inclined to consider that everything about Nature that can be known is already known. So it always has been. It is sufficient to read something of the works of Newton's contemporaries to see that even then many considered that with the discovery of the classical laws of mechanics there was nothing more to be learned about inanimate nature. Although this is very often contrary to our sense, we must not make the same error—to believe that no new discoveries will be made in the future.

You may possibly ask what these discoveries will be? If I could predict them, they would no longer be unexpected and new. Yet, all the same, I want to mention an instance when it seemed that I might have made a discovery, but I did not try to do so. In this example there is an instructional element.

When in the 1930's I produced very strong magnetic fields, ten times more intense than any produced before me, scientists counselled me to carry out tests on the influence of strong magnetic fields on the speed of light. Einstein talked with me most persistently of all. He said, "I do not believe that God created a universe such that the speed of light is independent of everything within it". In such cases Einstein loved to refer to God when there was no more sensible argument. From the tests which had already been made in this direction, it was known that if I were to carry out testing with my stronger fields, the effect would still be very small, only of second order. Here, of course, it was impossible to predict the true magnitude of the effect since the phenomenon was new. At the same time the test promised to be exceptionally complicated since similar experiments had already been carried out with fields up to 20,000 oersted, and they showed that even with a very sensitive measuring method the magnetic field does not influence the speed of light.

Another person who was pressing for this experiment and who had even offered financial backing was Oliver Lodge. He also turned to me with advice to carry out this exceptionally difficult and delicate experiment. Yet all the same I refused. Why? I will explain this by the following instructive example, which, perhaps, is not known to many.

As you will remember, the law of the conservation of matter was experimentally discovered by Lomonosov in 1756 and slightly later by Lavoisier. At the beginning of the present century Landolt verified it with great accuracy. He too placed the substance in sealed vessels, weighed it exactly before and after the reaction, and showed that the weight remained unchanged with an accuracy of up to the tenth decimal point. If one takes the energy which is released in the chemical reaction and calculates the weight variation by Einstein's equation in the theory of relativity, it appears that if Landolt had carried out his experiment with accuracy of two-to-three orders greater, he might have been able to observe a change in the weight of the reagent.

Thus we know that Landolt came very close to discovering one of the most fundamental laws of Nature. But let us suppose that Landolt had expended still greater effort on this experiment, that he had worked at it for five years more, had raised the accuracy by two-three orders, and he had observed this change in weight, the majority of scientists would still not have believed him. It is common knowledge that any experiment with limited accuracy is always unconvincing and that to check it, another experimenter has to be found ready to spend another ten years of intensive work on it. Experience suggests that so long as a problem's solution is at the limit of accuracy by known experimental methods, it can be verified only when Nature suggests a new method of solution. On other occasions it has been so: Einstein's law was verified fairly simply by Aston when he invented and developed a new precise method of determining the mass of radioactive isotopes from the deviation of an ionic beam. So we should wait, and in the case described by me, when Nature offers us new procedural possibilities for studying the influence of magnetic fields on the speed of light, probably, a simple and convincing experiment will become available for studying this phenomenon. That is why I refused to conduct these complex experiments.

Concluding this section, I think that one may say with complete confidence that in the not too distant future physicists are in for discovering very much more that is new and interesting and it is appropriate to quote the words of Hamlet "There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy". As it was three hundred years ago in Shakespeare's time, so it is now, and so will it always be. In essence, it is a question of nothing other than the law of continuous dialectical development of man's cognition of Nature.

MOST IMPORTANT SCIENTIFIC PROBLEMS OF THE NEAR FUTURE

Besides discovering new natural phenomena which we are unable to predict, scientists will also direct principal efforts to a deeper study of the natural phenomena already discovered, and to solving methodological and applied problems.

To find the lines of scientific research which will be foremost in the near future, one needs to define the fields of natural science which are most closely associated with pressing demands of practice.

The more important and interesting problems confronting science right now are well known to all, and I shall not dwell on them in detail. The first of these problems is the conquest of outer space. To achieve this aim the main thing has been done—man has been shot up from the gravitational field of the Earth; the main problem in the future is the use of nuclear energy as the motive force of space vehicles. What practical results will come from solving this problem? The possibility of populating other planets is increased. This, of course, is a task of the very distant future; we are not as yet even crowded on the Earth. But the beauty and fascination of penetrating hitherto unexplored domains is that man cannot foresee what he will find there. The whole historical store of experience unfailingly shows that new opportunities are always opened up for a rise in human civilization by exploration. Undoubtedly this will always be so in this case.

One may remark that right now there are already practical possibilities for utilizing space rockets—for disposing of the radioactive wastes and slag left from atomic reactors. More than once it has been pointed out that in the near future large quantities of radioactive waste from atomic electrical plants will start to accumulate and disposal will be difficult and dangerous; many consider that this will be the main obstacle to large-scale development of atomic engineering. If these radioactive slags are despatched into outer space on rockets, this will be quite safe for mankind and, apparently, it involves no great expense. It may be the solution to get rid of radioactive waste in this way.

One of the permanent, most important problems of the present and future is the production of cheap electric energy.

A most important possible solution of this problem is the controlled thermonuclear reaction. This is the most important problem of modern physics; it will give people an inexhaustible source of energy; the solution depends on creating a sufficiently high-temperature plasma. Scientists are still searching for ways of solving this problem.

A more immediate problem is that of using the heat from fuel efficiently. As you know, coal is converted into electricity in successive stages: firstly the chemical energy is converted into heat in a furnace and then the heat is converted into steam in boilers, then into mechanical energy in prime movers, finally, into electric energy in the generators. But in this way only 30–35 per cent of the coal's chemical energy can be used, and even this involves heavy capital investment in machines. To make this process more efficient in the future, a new line of development is envisaged. I have in mind the so-called "hydro-electromagnetic" generators. The idea of these generators was proposed at the beginning of the century. If a fast-flowing jet of a good conducting plasma is passed through a magnetic field, a transverse electromotive force arises. By using this force one can produce current and so convert the kinetic energy of the jet into electric energy. Recently, thanks to the development of jet aircraft and rocket engineering, the process of producing great jets at high temperatures has been well mastered; therefore realization of the old idea of the hydroelectromagnetic generator has become realistic and large institutes both with us and in the U.S.A. are seriously working on this right now. One may forecast that this generator will be operating effectively as soon as large outputs of the order of several hundred megawatt are attained; the whole installation will be relatively small in size.

Yet all the same the direct conversion of chemical energy into electric energy must be more alluring. In conventional galvanic elements and accumulators this has already been carried out and, as is well known here, an almost complete transition of chemical energy into electric energy is possible, and theoretically the efficiency may be close to unity. The basic problem facing scientists is to create a galvanic element where electric energy would be obtained directly from the oxidation of coal. The realizing of gaseous elements of this kind which will operate at high temperatures and pressures has appeared possible, and here during the last decades there have been notable successes, although the problem has not as yet been brought to a practical solution. Unfortunately, it is doubtful whether in general one will succeed in realizing similar plants for large outputs, since this is bound up with difficulties of principle. The point is that the chemical oxidation of gaseous elements has to be carried out on the surface of electrodes, and not in the volume of the electrolyte, whilst under these conditions a very extended surface is required for large outputs, which is practicable only on a large scale. Therefore this principle of generating electric energy will only have significance for small-scale power supplies.

One must never forget one problem in power engineering: the direct conversion of chemical energy into mechanical energy. Here people are lagging behind Nature. The muscle motor is still the most extensive. The quantity of mechanical energy producible by the muscles of all animals is still several times greater than the energy from all the heat engines created by people. The muscle motor, as shown by experiments, also operates very effectively with higher efficiency than engines, turbines or other heat engines. But most surprisingly, one has to confess, until recently scientists have not understood the essentials of the muscular process. There are many hypotheses, but until scientists have succeeded in artificially reproducing the contracting of muscle fibre, it is impossible to consider this process understood. Doubtless, research into the mechanism of muscular contraction will be one of the key problems in future scientific investigations. Physicists, chemists and biologists will take part in this work. What are the chances of the work being completed in the years immediately ahead? The problem can be considered solved if we succeed in reproducing the reversible contraction of synthetic fibre under the influence of a change in the properties of its surrounding medium, i.e. if we can simulate muscular processes. Today the number of types of links of chains which form the fibre of synthetic polymers is continually increasing. The nature and the properties of molecular bonds in polymers are beginning to be understood more and more deeply, and all this gives hope that the secret of muscular contraction will be laid bare on artificial fibre. In any event this is one of the most important problems of the near future and, by solving it, an effective mechanical motor will be placed in people's hands. It is natural to expect that such a motor will be portable and only suitable for small outputs.

Incidentally, many other secrets are hidden in the fibres of polymers which would be very useful for people to know. For instance, it is well known that an electric impulse (signal) can be freely propagated along nerve fibres which we consider to be dielectric. We know that this undoubtedly takes place in nerve fibres, but the mechanism of this interesting phenomenon is as yet not understood at all, and we do not know how to reproduce it. When this problem also has been solved, we shall be able to make signalization circuits, counters and other elements of cybernetic machines without metal conductors. To understand this phenomenon is also one of the great problems of the future.

SCIENTIFIC METHODS IN THE FUTURE

There are many very important scientific and scientific-technical problems which we cannot solve right now owing to the limited methodological possibilities available to us. Procedures may be limited by the complication of the phenomena itself, which leads to mathematical problems of insoluble complexity, or of the available method of measurement; the measurements may be insufficiently sensitive or too imprecise, or in general the phenomena may not lend itself to measuring at all.

Success in solving many problems therefore depends on expanding our methodological possibilities. In the main, success here depends on inventing new methods of observation, inventing measuring apparatus operating on new principles and, finally, by inventing methods for theoretical and mathematical generalizations of scientific experiments.

All these inventions can be regarded as scientific discoveries of their own kind; the greatest of them are made as unexpectedly and as unpredictably as scientific discoveries, and they also are manifestations of man's genius. Great methodological inventions, like scientific discoveries, can also lead to the creating of a whole scientific field and lead to the solving of basic problems which have long been confronting science. As an example of the latter one may point to Newton's invention of differential calculus, or Huygens' balance-spring for clocks.

One of the greatest of such modern methodological inventions is the creating of electronic cybernetic machines which has greatly moved many fields forward. The rapid development of cybernetic machines which is taking place right now is making it possible to solve many problems of great importance which until very recently lay beyond the bounds of accessibility (rapid and exact calculation of space vehicle trajectories, atomic, molecular and crystal lattice structures and many other problems). Doubtless, electron-cybernetic machines will intensively develop in the future, and with their aid many other important problems will be solved which have hitherto been beyond our reach.

Right now I want to turn your attention to some most important problems which it is necessary to solve and of which we need to have the theoretical solution; but at present they are so complex that they are having to be solved in a rough empirical or semi-empirical way. One needs to remember that empiricism has far from spent itself as a scientific research-method.

The using of empiricism in these investigations usually involves laborious collecting of great quantities of test data and great complications in systematizing and utilizing the data. Let us take as an example empirical investigations of a kind which are often now carried out, the problem of creating substances with well-defined mechanical properties—strength, heat-resistance, ductility and so on. It is well known that in the domain of the achievements in aviation, cosmonautics, turbine engineering the strength and heat-resistance of the materials are usually the chief limiting factors. It would be enough to raise the heat-resistance of alloys by several hundred °C and their ultimate strength by 20–30 per cent—this would enable many new technical problems to be solved. However, despite the fact that all the mechanical properties of metals are being measured well and rapidly, there is as yet no quantitative theory which links these properties of matter with its chemical composition and physical structure, although the nature of the forces between the atoms is well known. The mathematical problem is so complex that it cannot even be formulated. Therefore the main way of searching here is empiricism.

Yet it can be shown without difficulty that even empiricism cannot completely solve this problem. About 100 elements are known which form alloys. Let us assume that it takes one page to describe the necessary properties of one metal or alloy—its strength, heat resistance, elasticity, electro-conductivity and so on. To describe the properties of the elements themselves 100 pages are required; to describe the binary alloys 10,000 pages are required already. The alloys of ternary systems already fill a million pages. It is easy to see that to investigate and systematically describe the ternary alloys is the limit of possibilities. Thus the empirical method of study has inherent limits. The invention of cybernetic machines of the tabulator type naturally extends our possibilities here also, but all the same one needs to recognize that the problem of scientifically creating new alloys with specified properties has not been solved for more than three constituents. Though it is common knowledge that alloys of three or even more constituents are already used in practice and important problems have already been solved by such alloys.

Will this always be so? I do not think so. These multi-constituent alloys may, perhaps, be found by chance, but more probably by the intuitive "flair" of the talented scientist who, like a master chef, knows how to cook more tastily than the others. If there is intuition, this means there is also justification. It is the job of science to elucidate these justifications, though the method of solving such complex problems has not as yet been found, and this, undoubtedly, is one of the problems of the future.

There exists one other, less known problem which will be of great interest in the near future—it also is being solved empirically at present. It is the creation of superconductor alloys at normal temperature and with a sufficiently high critical magnetic field, i.e. the field which breaks this superconductivity. It is well known that in superconductors electric current flows without loss and, therefore superconductivity is already beginning to be widely used for designing high-quality radio-oscillatory systems, for coils with strong magnetic fields, for miniaturization of storage devices in electronic computers. But the main difficulty with the practical use of superconductivity is that all these devices operate at the very low temperature of liquid helium (4·2°K). Therefore most practical value would attach to the discovery of materials possessing superconductivity at room temperature. This would cause a revolution in modern electrical engineering since it would enable power to be transmitted without losses. But meanwhile the theory indicates that superconductivity cannot exist in pure metals at temperatures above the Debye temperature and, consequently, at present one can only await the discovery of such materials in the alloys, the theory of the superconductor properties of which is not as yet understood at all. Here also arises the problem of empirically studying multi-constituent compounds, about which I have already spoken.

One of the biggest problems to be solved in solid-state physics is the creation of polymers with specified properties. Polymers in living nature are always the main "building material" which performs functions of all possible kinds. Our age will not only be the age of the utilization of atomic energy, but also the age when man learns to produce polymers and to employ them as widely in practice as the fundamental "building material".

The variety of polymers is unlimited, there may be more of them than the alloys. The mechanical, electrical and magnetic properties of polymers are also diverse. Science is confronted with the problem of creating polymers with specified properties. Here also empiricism is inadequately powerful, as in the example of the alloys. Possibly, owing to the great regularity of polymer structure, the theoretical generalization will be found sooner for them than for alloys, which will save all empirical research into finding polymers with specified properties.

Successes in inventing new methods of experimentally investigating natural phenomena have been exceptionally big during recent years. Apparently, one has to attribute this to the rapid development of theoretical and industrial electronics and to those new possibilities which the development of nuclear engineering and physics has opened up.

It is interesting to mention that today frequency variations are being measured accurately to the 16th decimal point (using the Mössbauer effect), and time to the 11th decimal point (by molecular generators). Using electron beams and different methods of magnification, one can see the molecules and so on. Such achievements in inventing new methods of observation and measurement are now going on as in an unending stream. There are no signs that the development of our experimental possibilities will cease in the future. The next achievements in inventing methods of observation or measurement are as difficult to foresee as the new discoveries. But all the same I would like to point out a field of measurements where man is lagging behind Nature and where one ought to expect new inventions in the near future.

Today a physicist has instruments at his disposal which are many times more sensitive than our sense organs. A microphone hears better than the human ear, a photo cell sees better than the eye over a wider spectral region. Seismographs are more sensitive than our sense of touch and, of course, man determines temperature quite badly compared with a thermometer.

Only one feeling—this sense of smell, i.e. the determination and detection of small quantities of impurities of organic matter, is more perfect in the animal than by existing instruments.

Now let us take connoisseurs of wine. The great physicist Langevin was one. I well remember—it made a great impression on me—how once before dinner at a congress in Zürich in 1925 he tried the wine and once again correctly determined not only the brand of the wine from its taste, but also its vintage. He was an acknowledged connoisseur and he was very proud of this, perhaps, even more than of his successes in physics. But there are no such physical instruments that could do what he did even approximately.

The most sensitive method for determining inorganic impurities at present is considered to be radioactivation analysis. In this way one can detect impurities in amounts of one in 10^8 to 10^9. Comparing this with the smelling of a dog, the dog appears to detect much smaller quantities of impurities and in this case the dog's senses can identify them. One asks why man has not devised other instruments such as could catch the negligible quantity of atoms accessible to the smelling of the dog?

It is well known that the olfactory organs are the most complex of all the sense organs and the nature of the phenomena on the basis of which they function, has not as yet been discovered. Thus, to "surpass a dog's smell" is one of the problems of physicists in the future.

One may give the reminder that there is another field where Nature has invented a better mechanism than man. This is the mechanism of memory of the brain. This mechanism is many thousand times more compact and effective than the storage devices in modern computing machines. The nature of the mechanism of memory of the brain is also unknown to us.

All these problems are problems of the future, and here physicists must take away Nature's interesting and fascinating secrets.

THE FUTURE OF BIOLOGICAL SCIENCES

Many big problems in the biological field associated with queries in agricultural engineering, zootechnics and medicine are well known, and I shall not dwell on them.

In the foregoing sections I have indicated some of the biological problems which it is important to solve in the future—the nature of muscular contraction, propagation of electric signals in nerve fibres, the mechanism of memory of the brain and the mechanism of smell. The successes which have been achieved in solving these problems are indoubtedly due to the penetration of physics and chemistry into biology which is going on right now. The investigations give full grounds to suppose that not only will these problems be described by known laws of inanimate nature, but that these processes can be reproduced artificially and used in practice. By finding out the mechanism of these processes not only is the biological significance revealed, but physics and chemistry is also enriched. Therefore in the future we can expect a fuller merging of physics and chemistry research with biology.

The fuller understanding of biology's own very complex processes will undoubtedly deepen our knowledge of inanimate nature. Therefore the time has now come to speak of the favourable influence of biology on the development of physics and chemistry. The development of the chemistry of polymers and the study of their physical properties which is going on so intensively right now are instances of this favourable influence. For example, from the structure of the polymers used in Nature it is seen that Nature is a better "design engineer" than man, and at present there is much for us to learn. One ought to mention that in some respects scientists have surpassed Nature by creating processes such as do not go on in Nature. Thus, for example the uranium chain reaction, used for producing nuclear energy, does not go on in natural conditions. We can with great confidence assume that in the near future the solution of the great biological problems will be determined by the development of "composite problems", i.e. problems involving not only biologists, but also chemists, physicists and even mathematicians. Thus the expansion of big collective research projects about which I spoke at first, will also apply in solving the problems of the biological sciences.

One of the most important and interesting composite problems in the field of biology, where physicists, chemists and mathematicians have to participate, is genetics. The immense successes which have been achieved in genetics in recent years are already widely known. Today scientists have not only learned to produce artificial mutations, but have begun to acquire a detailed understanding of their physical essence. This has become possible thanks to determining the structure of chromosomes, the deciphering of the code by which in the genes the information is stored necessary for the development of a particular organism and, finally, to understanding the mechanism itself of the process of reproduction.

Of course, the main applied problem facing geneticists is to change the form (species) of organisms according to the demands of practice. As yet there is still a long way to go before these tasks are implemented, but ways of solving them are envisaged. The present means of influencing chromosomes—by irradiation or chemical compounds—can only produce random mutations. As yet one can produce desirable mutations only in the most simple of organisms—viruses, microbes, fungi. All these organisms rapidly multiply and the number of possible mutations in them is not so large; therefore, by working out an effective method of artificial selection one can create a species with certain properties for applied purposes. Precisely in this way they are now producing the most active antibiotic drugs. By irradiating fungus cultures they cause mutations in them and select those exhibiting the most activity in relation to a specified bacillus. But, of course, by such methods it is impossible to produce desirable mutations in complex organisms, just as it is impossible to improve a complicated mechanism by hitting it at random with a hammer.

But if scientists succeed in finding a method of producing mutations in desired directions, then, of course, man will have in his own hands a method of changing species on a scale which will be incomparable more potent than is now possible by selection and hybridization. Here due research is imminent. It is not hard to predict that the know-how to alter species will first be acquired on the simplest organisms, and then be extended to the more and more complex.

Let us suppose that scientists ultimately succeed in finding a method of producing artificial directional mutations which will alter the human species. An interesting and very controversial issue will then arise—the issue of actually altering the human species. This opens up the prospect likewise of changing the structure of society as described by Aldous Huxley in his daring fantastic utopia *Brave New World.* The history of civilization teaches us that eventually the fantastical becomes real.

But this is a question of the distant future and for the present one ought hardly to dwell on it. For the present it is interesting to pose the question: what are the chances that in the next few years a method will be found for "controlling" the direction of mutations? One has to say that we are still very far from solving this difficult problem. Possibly by studying the physical properties of synthetic polymers one might discover a method for more organized action upon their chain structure. It is hard to say how soon this will be done, but in any case the problem is now actual in character, and it lies within the framework of the laws of physics and chemistry which are known to us.

It is hard not to point out that what has seemed the more complex problem—hereditary and direct action on the mutation of species—has appeared to be understood sooner than the other properties of animate nature, and, possibly heredity will be the first to be "controlled" by man.

In conclusion I want to pose the following other fundamental question associated with living Nature and which is imminent.

The question is the following: is our present-day knowledge of the laws of inanimate nature adequate for describing all the phenomena which are characteristic of the living world?

We know that most phenomena are described by existing laws, but all the same I dare say that one of the main properties of living Nature—to reproduce itself—may be the manifestation of various forces in Nature which as yet are still unknown and unexplained by the known laws of interaction between elementary particles. We have no data to affirm that no new property can appear in chains of sufficient length by the arrangement of the atoms according to definite rules which is analogous to that of self-reproduction in living Nature. Of course, in individual atoms and simple molecules such a property may be unnoticeable. That this possibility is not excluded, we can illustrate by the following example.

It is known that only with large accumulations of elementary particles does the force of attraction begin to play its role between them, the force which we call gravitational force. You see, the nature of gravitation is not considered in describing quantum and electrical interaction of atoms and it manifests itself in Nature only in large masses. Likewise other as yet still unknown properties of atomic interaction may be manifest only when their mutual arrangement is ordered.

It is science's task by experimenting to elucidate these laws of self-reproduction and to find those parameters by which it will be possible to describe these laws quantitatively. If this succeeds in being done, new properties of the nature of matter will be discovered which escape us in studying the inanimate world.

But the approach to the solution of this problem is not yet even established, and we can only relate its solution to the distant future.

THE FUTURE AND THE SOCIAL SCIENCES

There is one other field of science on which I shall touch only briefly—this is the science of human society. The social sciences are special in that they have their own theoretical methods of study and generalization which differ from those adopted in the natural sciences for elucidating conformities to law. Owing to this there is a distinct boundary between the natural and social sciences which divides them into two independent and isolated fields of knowledge.

It is generally acknowledged that the basic science of the laws of the development and construction of human society—historical materialism—was founded by Marx. He first elucidated such basic parameters as the class structure, labour productivity and so on, by which one can characterize the structure of society and by which the laws of its development are determined. Marx, Engels, Lenin and their followers created the scientific basis for building a new kind of social structure—communist society.

To the social sciences one ought also to relate the science of man's higher nervous activity. The founders of this basic science are considered to be I. P. Pavlov and Sigmund Freud. They were the first to place the study of thought processes on an experimental basis. By them were found the laws of man's perception of the external medium, the emergence of conditioned reflexes and the influence of the subconscious on man's activity.

The outcome of these investigations and also of those of their pupils is made use of widely in pedagogics, psychiatry, legal practice, etc., and in capitalist countries also for advertisement and propaganda. With the development of the science of higher nervous activity, it is becoming still more closely tied with the social sciences.

It is natural to suppose that just as the successful development of biological sciences must be based on physics and chemistry, so also the science of the laws underlying the organization of society must be based on the science of man's higher nervous activity. Only on this scientific basis can organizations be created for the right education and training of people. Only on this scientific basis can the right forms of organization be sought for man's toil and leisure.

And—this is the main thing—only on a scientific basis can a healthy and effective structure of society be created. We must learn to build the State apparatus on the basis of the science of society, and it is necessary to learn to analyse it just as engineers now analyse electrical machines; it must be simply constructed and operate with a high efficiency. The value of the social sciences for mankind is quite obvious.

It is natural to pose the question: why even in our time which by many is called the time of the scientific-technical revolution are the social sciences evolving so feebly? I daresay that the more natural explanation of the hold-up in the development of the social sciences is, as in all such cases, the unfavourable conditions existing right now.

I take the liberty to explain this thought by a somewhat frank analogy which explains the state in which the social sciences find themselves today.

I daresay that the external conditions which have been created today for the social sciences are somewhat similar to those in which the natural sciences were in the Middle Ages. It is well known that the main brake on the development of the natural sciences at that time was the scholastic environment. At that time the Church took upon itself a monopoly of scholastic-dogmatic interpretation of all natural phenomena, decisively sweeping aside whatever in the slightest degree contradicted the canonical writings. This retarded also the development of the natural sciences. Only three hundred years ago did the natural sciences become their own master away from the Church and start to develop rapidly, and the pace of this development goes on building up to this day! Right now in the world there is a great variety of State structures which only aknowledge that to be true in the social sciences which demonstrates the expediency of these structures. Naturally, under such conditions the development of the social sciences is greatly hampered.

Let us take as an illustration how the social sciences are taught to youth in capitalist countries.

There, in secondary schools, as everywhere, the same laws of mechanics of Newton are taught, but there in school they do not teach historical materialism and the laws discovered by Marx on the basis of which capitalist society functions. You see the laws of development of society must also be the same everywhere. The laws of Marx are just as universal as the laws of Newton. Why then in the social sciences have not the only laws been acknowledged?

The answer is clear: there is no objective approach to the science of society. Until this is created, the social sciences will develop with great difficulty.

This, it seems to me, explains the striking contrast which exists today in the scale of development of the natural and social sciences.

I daresay that the time is not far distant when people all over the planet will realize the necessity of recognizing the same laws of development of society and on this basis will set up for themselves the more effective and just social system.

It is well known that the technical possibilities which right now are deposited in man's hands by modern science are leading to such sharp contradictions between States that in the face of the possible ruin of the people of all the world they will come to a conclusion regarding the necessity of a scientifically substantiated and effective social system free from war.

It is not hard to predict that the development of the fight for peace and the acknowledgement of the danger of the present deadlock into which mutual relations between different social systems have fallen, inevitably must lead to an intensive development of the social sciences in the near future.

PLATE 23. Niels Bohr.

27. IN COMMEMORATION OF NIELS BOHR

NIELS BOHR departed from life. Physics suffered a heavy loss. In our time, there has been no man of such an impact on natural sciences as Bohr. Of all theoretical paths that of Bohr was the most important. The death of Bohr is a particularly sad event for Soviet theoretical physicists, who were closely connected with Bohr and his school, and this refers in the first place to L. D. Landau—a direct disciple of Bohr.

Nowadays scientific research is carried out by large collectives of scientists, in which all of these are connected with one another. Bohr formed such a collective of theoretical physicists on an international scale. He was a great scientist, who knew how to reveal in his disciples their creative ability. This required not only a broad knowledge but also outstanding human qualities. The altruism, criticism and indulgence to somebody else's mistakes are Bohr's features well known to all of those who were his disciples or worked with him. He was not only a great scientist but also an outstanding man. He was in sympathy with advanced political views and was himself of a progressive influence. He was for many years a honorary member of our Academy of Sciences.

When the Germans occupied Denmark, Bohr escaped from the fascist regime—the most reactionary regime mankind saw. Afterwards, together with all progressive minded people, he took part in the fight against fascism. For the rest of his life he was an active fighter for peace and international cooperation of scientists.

His departure is bemoaned not only by his disciples but also by all progressive-minded scientists the world over.

П. Л. Капица, Памяти Нильса Бора, *Природа*, **1**, 67 (1963).

Address to the Scientific Council of the Vavilov Institute of Physical Problems of the Academy of Sciences of the USSR.

28. THE PHYSICIST AND PUBLIC FIGURE PAUL LANGEVIN

Paul Langevin was not only an outstanding physicist, but also a great progressive public figure and friend of the Soviet Union. In 1924 he was elected corresponding-member of the Academy of Sciences USSR, whilst in 1929 he was made honorary Member of the Academy of Sciences USSR. He was also an honorary member of different academies and scientific societies in a number of countries; in 1928 he was elected a Foreign Member of the Royal Society of London, and in 1934 he was elected to the French Academy of Sciences. This honour he merited as a physicist for exceptionally important scientific research.

His main works relate to theoretical physics and the greatest of them to magnetism, where his work still has value and is considered classic, and to acoustics. Langevin found the method and then produced ultrasonic waves, and he first proposed using the phenomenon of piezocrystals to excite short acoustic oscillations. A whole new field of science and engineering has grown up on this basis.

It must be said that Paul Langevin's influence on the development of world physics has been very great and is not confined to these two fields. Langevin was also a great teacher, under him were many pupils, of whom two have received world recognition, one of them de Broglie and the other Joliot-Curie. Although Langevin used to publicize his works comparatively little, yet he was a very generous teacher, gave ideas, inspired and backed up his pupils. In this respect his influence on French physics—if an account could be drawn up, but which to do unfortunately is not possible—is certainly no less, but even greater than that of the works which he did publish.

I saw rather much of Langevin, I was lucky to gain his friendship and now I think of him with exceptionally warm feeling.

The image of Langevin can, I think, be described in a word: he was a man serving in everything progress; he was progressive in science, progressive in his political views, progressive in his philosophical views, and progressive in his social activity. This progressiveness runs through all his life. The culture of mankind grows, science moves forward, the social system develops and our philosophical representations of the inter-relation between man and the

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П. Л. Капица, Физик и общественный деятель Поль Ланжевен, *Жизнь для Науки*, Знание, Москва 1965, стр. 54.

Text of a talk given to a joint session of the Academy of Sciences of the USSR and the Society "Druzhba" (Friendship) on 23 January 1957.

Plate 24. Paul Langevin (1872–1946).

material world become deeper. Whether we like it or not, all goes forward. People are divided into three categories: some stand in front and spend their powers to further science, culture and humanity—these are the progressive people. Others, and these are the majority, go along with progress, at the side, they do not interfere and do not help; and then finally there are people who stand behind and hold back culture—conservators, cowardly and without imagination.

For those who go ahead it is hardest of all, they make new paths for progress, all possible tests of fate befall them. One such was Paul Langevin, and fate made him face many severe tests. One asks why there are such people who choose this road, what compels them to go forward when it is more pleasant and quieter to go at the side, even if not to trail behind? It seems to me personally that there are two reasons. Intelligent men cannot be other than progressive. Only an intelligent man, endowed with boldness and imagination, can be progressive and understand the new and to what it is leading. But this is insufficient. It is also necessary to have the temperament of a fighter. When intelligence is combined with temperament, man becomes truly progressive. One such man was Paul Langevin. Most often in life we observe that only in youth does that temperament light up most and makes a person progressive; with age man wants a tranquil life, therefore youth, especially in student years, is the most progressive part of humanity. With Langevin this did not happen. He was a fighter for progress to the end of his life, and the older he became the more zealously he fought for progress. Now this extraordinary feature in him always struck me and called forth deep sympathy and respect.

Now I want briefly to consider his scientific work from the viewpoint of progressiveness.

His first works related to magnetism and were done in 1907. Here he was the first to apply statistical mechanics, developed and generalized at this time by Ludwig Boltzmann, to the laws of para- and dia-magnetism, which had been discovered shortly before by Pierre Curie, teacher and friend of Langevin.

Now we regard this as nothing out of the ordinary, but if we reconstruct the situation in physics at the time, we see that these works in their essentials were exceptionally progressive. In those years Boltzmann's ideas were coming into life with great difficulty. In 1906 Boltzmann put an end to his life precisely because the fundamental and bold idea on which he based his works in the kinetic theory of matter—the relation between entropy and the probability of existence of molecular states—was not understood or recognized. The leading scientists of the time, like, for instance, Oswald, did not want to recognize at all the atomic theory, and a storm raged round Boltzmann's works. Langevin's approach in his first works on magnetism was based on precisely these new Boltzman ideas.

Slightly later, or at almost the same time, Einstein's work on the theory of relativity appeared. It was published in 1905. Already 50 years have elapsed since then, and now only the most hardened conservators object against the main ideas of the theory of relativity. But when it appeared, then, of course, there was a stream of objections and the biggest objections, of course, were against the law which for the first time had been formulated perfectly clearly and quantitatively—the law of the equivalence of mass and energy. This equivalence is defined by the law according to which the mass of a substance, multiplied by the speed of light squared, can be converted into an equivalent quantity of energy. Some scientists in this saw a violation of the law of the conservation of energy and of the law of the conservation of matter—the basis of physics of those days—and this caused a storm of objections.

Langevin was one of the first to publicize with great energy Einstein's ideas in France. Almost at the same time as the discovery of Einstein's law, he had a paper printed in which he indicates the possibility of a difference between the deviation of integral mass numbers in a periodic system from the integral hydrogen multiplicities associated with the appearance in complex atoms of an energy surplus which increases their atomic weight. Now we know that this was the right prediction which only a great scientist could have made, and it has since been verified experimentally more than once. Now these views have a precise theoretical justification. This again shows how Langevin picked up new ideas in science and how he put them into practice. Of course, now we have the atomic bomb which has demonstrated to all mankind the explosive force when matter changes into energy. Simple calculations show that in an atomic bomb only one gram of matter is converted into energy, whilst in the hydrogen bomb no more than one kilogram. Of the possibility of such a colossal effect in the change of matter into energy, Einstein, Langevin and other leading physicists spoke more than once, but there were more than a few of the others who did not believe it. A clearer demonstration of Einstein's law than the bomb explosions in Hiroshima and Nagasaki is hard to imagine. Yet, despite this, we in the editorial office of *Journal of Experimental and Theoretical Physics* still to this day are receiving articles which attempt to refute the validity of the theory of relativity. In our days such articles are not even considered, clearly being anti-science. This is a second example of how Langevin fifty years ago travelled the right and leading road in physics.

The third instance of his progressiveness in modern physics I observed personally. In 1924 I went to Langevin in Paris. Then he was professor at the Collège de France. When I arrived he immediately said "My pupil de Broglie has done remarkable work, I want him to tell you about it". He called de Broglie and asked him in my presence to tell me of his new work on the wave nature of electrons; as is generally known, this work has since become a classic. Then I saw how Langevin was carried away by this work. Quite possibly, without the backing of Langevin, de Broglie would not have acted towards his new idea with the boldness which was necessary to develop it and put it into practice.

That at the time his idea caused great scepticism, can be illustrated by the following example. When I came back from Paris to Cambridge I told the theoreticians about de Broglie's work. Paul Dirac was then still a student; he was taking a short course on magnetism which I was then giving; he sat in the front row and I did not suppose then that he would become the great scientist to find the most general mathematical expression for de Broglie's idea. The principal theoretician in Cambridge then was Fowler. Neither he, nor his colleagues, wanted to recognize de Broglie's views and take them seriously. And when I suggested supplying a seminar paper on this theme, they said to me: "We will not waste time on this". A year or two later when Schrödinger did the work in which he mathematically generalized de Broglie's idea, and his significant, now classic, equation appeared in which he showed that what de Broglie had done was nothing more than the eigenvalue of functions in known equations, the fundamental importance of de Broglie's work became clear to all.

The story of how Schrödinger produced his equations is very instructive. Schrödinger at the time was working with Debye and he told me the details of how Schrödinger produced his equations. Reading through de Broglie's paper in the *Comptes Rendus*, Debye suggested to Schrödinger that he should discuss it at the seminar. Schrödinger replied approximately so: "I do not want to talk about such nonsense". But Debye, as his senior on the staff, said that it was necessary for him to do it all the same. Schrödinger had to agree and he decided to try to present de Broglie's ideas at the seminar in a more comprehensible mathematical form. When he had succeeded in doing this, he had arrived at the equations which made him famous throughout the world and which now bear his name. Debye told me that when Schrödinger gave his lecture at the seminar he himself did not understand what a great discovery he had made. Debye there and then at the seminar said to him "You have done a remarkable work". But Schrödinger himself thought that he had only found a good method of telling a group of physicists what de Broglie had done. And this took place two years after de Broglie's work appeared. Langevin immediately, from the very first, and earlier than all, understood that a new field of physics lay in the ideas of de Broglie. This example once again shows Langevin's amazing feeling for everything progressive. It always struck me in conversation with Langevin how he was able to see the scope of what was happening in science; one really should bow to his perspicacity.

In the social field Langevin was just as progressive as in physics. He often said with pride "I was born in Montmartre". As is generally known, this is the most proletarian part of Paris. His grandfather was a simple metal worker, his father a land surveyor. Langevin himself was born in 1872 in fairly poor conditions, went through the municipal school and then obtained a higher education by grants. He was a widely gifted person and, of course, he did brilliantly. Then he became a pupil of Pierre Curie. Pierre Curie sent him to the Cavendish laboratory, where he worked in the same room with Rutherford. In those years Cambridge had long been the centre of physics. The Cavendish professor then was Sir Joseph John Thomson who became famous for discovering the electron; the research which he started on the passage of electricity through gas was in those years the leading field, like, for instance, nuclear physics in recent times.

At Cambridge, Langevin did his first experimental work and there his scientific career began. After this, for many years, Langevin maintained a great friendship with British scientists, especially with Rutherford. Langevin was a charming person, always gaining the favour of people of any class, and everywhere he had friends. On his return to Paris, Langevin started working in the Collège de France, where after his teacher Pierre Curie he occupied the position of professor. From his very first works on magnetism onwards he became one of the leading physicists in France.

He also began his political activity very early: he began it approximately in his schooldays. The beginning of his political activity is linked with the famous Dreyfus affair—a shameful court case started by a group of anti-semites whom one may describe as the predecessors of fascism. Then Emile Zola interceded for Dreyfus, writing the famous book *J'accuse.*

When Zola was subjected to victimization, Langevin stood out in his defence. This was his first action in the social field. He often, recalling this action, said "Yes, they were good times then, imagine, the fate of one man could interest all the earthly globe". After this followed a whole series of political actions: the list speaks for itself.

In 1920 at a meeting in the Salle Wagram in Paris Langevin in a brilliant speech stood out in defence of the sailors of the Black Sea squadron who refused to fight against the young Soviet Republic. Langevin with just the same keenness as in science foresaw the progressive value of our social revolution and at once openly took to backing it.

In the same year, when he was professor at the Collège, he stood out against the use of students as strike breakers during the transport strike in Paris.

Together with Romain Rolland and Henri Barbousse he unswervingly stood out against fascism.

He stood out in defence of Dimitrov during the Leipzig case.

He was one of the most active defenders of Ernst Thälmann. He was president of the "Ligue des droites de l'homme". He not only headed it, but was one of its organizers.

He more than once stood out in defence of the Spanish Republic. In this list the same words are repeated: he stood out, he stood out, but behind these words is concealed great social activity and great organizational work.

Langevin stood out with a blazing censure of the Munich pact and stood out against the arrest of 27 communist deputies at the beginning of the war.

When the war started the opportunity was presented to me to write to Langevin and invite him to the Soviet Union for the duration of the war. Knowing the hatred which the fascists nourished for him, his fate in France was terrifying and, of course, it was necessary to offer him the opportunity to go to a country where he would be in safety and could continue the struggle for France. In a letter he replied to me that he would come to the USSR with pleasure, but at the time he had to finish one more matter; in the University of Paris an anti-semitic movement was then starting and Langevin headed the struggle against it, and until this movement was liquidated, he did not feel it right for him to leave Paris.

When Langevin decided that he was able to leave Paris, it was already too late; Hitler's government refused to allow him through Germany. Paris was occupied by German forces and Langevin was immediately arrested. Two months he was in prison, then they sent him to a small town where he occupied the position of physics teacher in a secondary girls' school and held this post for the first half of the war.

Langevin's family was progressive and they were all fighters against fascism. Langevin's daughter was arrested and sent to Auschwitz, where she was interned for the war. The daughter's husband, Solomon, a well-known communist, was arrested and shot by the Germans. Langevin had to get out of France. This was no easy task—he was already close upon 70. He escaped across the mountains into Switzerland. An automobile accident was staged, he was bandaged up and as a casualty was carried in someone's arms across the mountains. He stayed all the second half of the war in Switzerland where to the extent of his powers he continued to take part in the freedom movement. When he learned of the death, i.e. of the shooting, of his son-in-law Solomon, he wrote Duclos a letter in which he joined the Communist Party to which Solomon had belonged. Thus from 1942 until his death on 19 December 1946 he was an active member of the Communist Party.

This short list of facts, it seems to me, gives quite a vivid picture of Langevin's social and political activity. And from this list it is seen that there was not one great progressive event in Europe and in France in which Langevin would not take an active part. But there were also other fields of social life where Langevin stood out, for example, his most lively participation in questions of peoples' education. I remember, one day in Paris, I said to Langevin I had to go to Strasbourg to give a lecture at Strasbourg University. Langevin replied "Very good, let us go together. I too am going to Strasbourg: I have to give a lecture there on the teaching of the French language in Alsace". I attended his lecture, just like he came to mine, and I heard how interestingly he analysed the question of the teaching of the French language in Alsace. This was no easy task since a complex political question was bound up with it, and, because the sympathies of the population were divided between France and Germany, Langevin had to speak with great tact. Listening to him, I saw how exceptionally skilfully he drew up his report.

Such is the picture of his activity. A man who was engaged in such progressive activity both in the field of social life and also in science, could not help but be attractive, more particularly for young people.

Langevin was 20 years older than I, but in spite of this age difference, it was exceptionally easy and simple to associate with him. He was an exceptionally charming person and was liked by the masses of France. All, in my opinion, liked him. I do not know a person who would not treat him kindly. Even people of opposite political views treated him kindly. Gentleness, exceptional kindness and sympathy overcame and subdued all. With any person, whether it was the Prime Minister or a student, he spoke just in the same way and both felt at ease.

As an example of different persons' respect for him, I shall quote the telegram which Einstein sent to the Paris Academy after Langevin's death. This telegram is very short, I chose it not because it was written by Einstein, but because it exceptionally well, in my opinion, expresses and concisely expresses in reality who Langevin was: "The news of Paul Langevin's death shocked me more sharply than many disappointments and tragedies which have happened in these years. How few people of a generation combined a clear understanding of the essence of things with an acute feeling for truly humane requirements and the ability to act energetically! When such a person leaves us, we feel an emptiness which seems unbearable for those who remain!"

In conclusion I would like to talk about a small trait in his character which imparts still more charm and humanity to his individuality. Langevin had one foible, he loved wine. Not in the vulgar sense did he love wine, but he loved the bouquet of wine, he loved wine as a connoisseur. He said "One doesn't drink wine, one talks of it". He has taken a bottle of wine, held it in his hands, inhaled its perfume and said that it was Burgundy of such-and-such a year, such-and-such a vintage, then the wine harvest was such-and-such and it was distinguished by such-and-such properties. He could recite a whole poem about a bottle of wine. He was proud of his knowledge of wine. This was his, as the English say, "hobby".

Once in Zürich during a conference I used to sit with him in a restaurant at the same table. Every time he very carefully chose a most rare wine and on the spot gave me a lecture on it. His knowledge of wine was not amateurish. The French wine-makers after the grape-gathering invited him to come and assess what the wine would be like in a few years' time. He went and he was very proud of the fact that the Burgundy wine-makers agreed with his opinion. But most of all he was proud of the fact that once in the Var valley in the south of France, when he was tasting wine, he "discovered" an excellent new wine. So from a plebeian red wine, on his valuation, a vintage wine was made.

And he genuinely rejoiced in discovering a new sort of vintage wine and he was very proud of this. The theory of magnetism, which was his greatest triumph, he did not mention, but he spoke with great passion of the new vintage wine discovered by him in the Var valley.

Well, here is my short sketch of the work and personality of this remarkable, progressive man. I consider it a great fortune for myself that I knew and loved this outstanding man and had many conversations with him.

29. EFFECTIVENESS OF SCIENTIFIC WORK

The question which I wish to pose is: how efficiently are our scientists and institutes working? Should we not direct our main efforts at improving the working conditions of the existing institutes and of their research workers so as to improve their productivity, rather than towards further growth of the Academy? Perhaps with the same expenditure in physical terms we may be able to gain more in this way. One may enquire by what factor the productivity of our scientists can be increased. It is of course impossible to give a precise answer, but I think there is still very great scope.

There are three main ways of influencing the work of scientists in order to increase their effectiveness. For the sake of brevity I shall call them morale, finance, and establishment. Undoubtedly the first is the most important. The attitude of scientists is largely determined by the choice of a right direction for his scientific work and its proper implementation. Unless a scientist has the right to disagree with one or more of his own colleagues as to the line which his own work should follow, he is no longer able to disregard the opinion of the scientific community as a whole. We must pay more attention to cultivating and maintaining social concourse amongst scientists in the Academy. This is one of the most effective ways not only of raising the level of scientific work, but it is also the main way of concentrating our forces on the more promising and chief lines of development. It is essentially this which is the real way to plan scientific work.

One means of social influence is to hold discussions on 'problematics' and particular themes at scientific meetings. It needs to be acknowledged straightaway that we are still not working hard in this direction. Meetings on narrower themes and conferences on special topics go well, but broader discussions do not usually proceed so well; the pulse of social life of the scientists attending them is very weak. The art of discussion and of scientific arguments has faded away in our country.

I have said many times that we need a club for scientists where we could gather and talk over urgent matters at ease. When I was in England the most interesting conversations on the burning topics in science at the time took place at dinner in the colleges. We discussed questions which embraced many fields of science, and this was the best way of widening one's horizon and so understanding the significance of some particular line of scientific enquiry.

П. Л. Капица, Эффективность научной работы, в *Теория, Эксперимент, Практика*, Москва, 1966, стр. 24;
From an address at the session of the Praesidium of the Academy of Sciences of the U.S.S.R., 4 December 1964.

It is necessary to develop social life in the Academy. In the epoch of the cult of personality this was difficult. But you see today there is again an opportunity for this! Why therefore is 'fraternity' still lagging, because this lag is by no means inherent in our nature.

An important factor in morale is to take part in scientific life abroad—at conferences and similar gatherings. As yet we still take little part in them. Our delegations are still only one quarter or one fifth as many as those of the U.S.A. and other countries, and often the delegates are selected by bureaucratic methods, and without strict regard to the criterion of scientific qualification and interest. This matter needs to be improved and without begrudging the means.

Another factor of morale which we do not take into account sufficiently, concerns our journals. A journal should be selective and provide constructive criticism. When an article is received, it is necessary to subject it to competent appraisal and notify the writer of the critique, in other words the Editorial Board of a scientific journal should carry on its work along with the author. We are engaged on this in the *Journal of Experimental and Theoretical Physics*, and although this is troublesome and sometimes difficult, yet I consider that this work is of great benefit for the direction of scientific research.

Let us now consider how the development of science is affected by the "establishment". When we in the Academy arrive at the conclusion that some field of science is lagging in our country, at once the question is raised about material support for some laboratory or even about the construction of institutes and so on. But it should be understood that it is impossible for us to maintain all fields on the same high level, so it is rather more correct to concentrate our efforts wherever we are powerful and where there are already good scientific traditions. Science needs to be developed in those directions where we are lucky to have a great, bold and talented scientist. It is well known that no matter how much you support an ungifted person, all the same he will do nothing great and purposeful in science. In the development of any particular field our first duty is therefore to proceed from a consideration of the creative forces of the person who is working in this field. You see, our science is a creative vocation, like art, music, and so on. It cannot be thought that by setting up a department for writing hymns and cantatas we shall get them: unless there is in this department of the conservatory a great composer equal in power, for instance to Handel, nothing will be produced. The lame cannot be taught to run, no matter how much money you spend on this. It is the same in science as well. The governing body of the Academy should seek out, attract and support the most talented people, and it should be engaged on this even more than on thematics.

In an institute the leading rôle in the selection of cadres should be played by the Director, who himself should be a great scientist. Yet this implies that he should enjoy great rights in this matter. But in general today the Director is largely fettered hand and foot both in financial and establishment questions. As I have pointed out, finance is the third way of influencing the development of science. Here at home there is money for research, the State is not stingy, and it is easier for us to obtain it than, for instance, for American scientists. But perhaps you remember the children's game about the Lady of the Manor. "She sent a hundred roubles, buy what you want", but then afterwards the rider follows—"buy nothing black or white, and do not say anything either", and so forth. Hence it appears impossible to spend the hundred roubles without tricks. A Director finds himself in this same position too: they give him money, but greatly restrict him in deciding how it is to be spent.

It is also generally known that a Director is greatly restricted in the choice of cadres, he cannot himself dismiss anyone, nor can he provide any incentive. The fettering of the Director, his restricted rôle in this important matter, is one of the reasons for the low efficiency of our institutes.

Unless the work of the Director of an Institute is made easier and simpler as a scientist directing scientific research, as previously it will be hard to attract scientists to this work. Today many outstanding scientific collaborators of existing Institutes (for instance, of our Institute for Physical Problems) would be quite able to head independent scientific collectives, but they do not wish to do so. If the work of the Director were made easier and simpler, this would of course be instrumental in a more rapid promotion of capable young scientists into directorships. It should be added further that the level of our scientific youth is higher than, shall we say, in America, where many creative and talented persons do not go into scientific institutions, but are attracted to commercial work which has much higher material rewards to offer there. Here at home this does not take place, but we are insufficiently using this advantage.

And the last thing which I should like to mention concerns the building of new Institutes outside the limits of Moscow. From my own experience it is more productive and quieter to work 50 kilometres from a large town. But to put this into effect, it is necessary to provide really good conditions, better than today in Moscow, both for scientific creativity and, of course, amenities. We have still not managed to achieve the latter. Scientific workers are therefore very reluctant to leave Moscow, whilst in this respect success cannot be achieved by compulsion.

In setting up Institutes outside large towns one should never begrudge expenditure on good living conditions, even if this is more than the outlay on the Institute itself.

30. ADOPTING ACHIEVEMENTS OF SCIENCE AND ENGINEERING

It is well known that the main index of economic progress is productivity, and increases in productivity are achieved mainly by adopting new achievements of science and engineering.

When the growth of productivity slackens, the reasons need to be sought in the failure of industry to adopt scientific and technical achievements.

From official statistics it will be seen that the rise of productivity in the U.S.S.R. has in the past reached 13 per cent per annum, but the pace has since slackened and in recent years it has dropped to 4–5 per cent.

This shows that the process of adopting new techniques in the U.S.S.R. is not now satisfying the demands of U.S.S.R. industry.

I want to dwell on the reasons why the adoption of achievements of science and engineering is lagging behind. It is generally known that in the U.S.S.R. the adoption of new techniques by industry always proceeds slowly and with difficulty. This will be seen from the word *vnedreniye* which we usually use in this connexion, meaning "introduction". We speak of the "introduction of new methods" and "introduction of the achievements of science". The word for "introduction" in the Russian language signifies that movement forwards meets with resistance from the environment. We are thus accustomed to new techniques meeting resistance in their adoption and for a long time we have been using the word "introduction" without noticing that we thereby characterize abnormal conditions in the adoption of new techniques. When we begin to use the word *dsudeniye* (adoption) one may suppose that normal conditions will have been achieved for adopting them.

My experience over many years in this field shows that six conditions have to be fulfilled for successful adoption of achievements of science and new technique by industry.

I shall enumerate them and then look at what is needed for them to be implemented in our country.

Adopting new techniques means that industry must learn to do what it has not been doing before. So adopting new techniques should be regarded as a process of study and therefore it needs to be carried out by the same pedagogic methods which are usually used whenever we teach somebody something new. When we are teaching students or schoolchildren, the main essential pre-con-

П. Л. Капица, Освоенье достижений науки и техники, *Теория, Зксперимент, Практика*, Москва, 1966, стр. 7; Address to the general assembly of the Academy of Sciences of the U.S.S.R., 13 December 1965.

dition is the person's wish to be taught. If there is no such strong desire for knowledge, it cannot be driven in by the cane. Does our industry always want to be taught what is new? Are the conditions here at home always created such that industry actually feels it is worthwhile for it to learn new techniques?

Obviously, for this wish to be present one needs to create favourable morale and material conditions. It is, of course, necessary to create those conditions under which industry and also the factories would have an interest in being taught what is new. They must feel that this is worthwhile to them, useful and honourable. It is this which is the No. 1 condition.

Condition No. 2 is that when we teach a person something new, this person must always have appropriate training. It is impossible to teach advanced mathematics if the pupil does not know algebra and trigonometry. Therefore when teaching something new the person being taught must have been trained sufficiently. In our country this is often not taken into account. I know of a number of cases where management has authorized the production of new apparatus at a factory which has not been prepared at all, and so despite every effort the factory executive has not been able to complete the order. For instance, one good factory was authorized to make special metallic vacuum Dewars even though the works had no experience of vacuum engineering. With difficulty the factory met this order, but it produced many poor quality Dewars and the adoption process took several years. At a factory prepared for this order, the work could have been done more quickly and more easily. Hence the second condition is adequate training of the pupil.

The third condition, also well known from the teaching profession, is that the pupil must never be overloaded with studies. Each factory, each industry, can only assimilate annually a definite quantity of what is new, even if there is adequate training and the desire to learn. But in our country it often happens that as soon as a works shows interest and starts to adopt something, they start to overload it out of proportion. One needs to remember that there are also limits to how much new knowledge can be assimilated by industry.

Fourth condition: when you teach somebody something, sufficiently favourable material conditions must be created. It is irrational to teach either a person or a factory entirely at their own expense in terms of internal resources. For teaching it is always necessary to provide a good material basis appropriate to the task posed. Simply speaking one needs to allocate sufficient means to whoever is being taught something new.

The necessity of these four conditions is easily understood, and it is comparatively simple to realise them in practice.

The fifth condition, less obvious and rather more difficult to implement, is the following. From practical teaching experience it is well known that if you are educating someone, it is always necessary to produce a clear programme as to the course of instruction. Similarly, if something new is being adopted in industry, for speedy success it is necessary to have a well-developed programme indicating the best way of proceeding. But, as a rule, little attention is paid to this here at home; they leave adoption to *laissez-faire* and even do not consider any kind of programme to be at all necessary.

In forming such programmes it is necessary to fulfil two conditions. Firstly the programme must take into account the production possibilities of the works and, secondly, it must have regard to the specificity of the new process which is adopted. At the works there is usually no single person able to see both these sides of the picture. So when a scientist or an inventor, or even a scientific institute, is entrusted with drawing up the programme, the specificity of production is not taken into account in this programme. When the factory itself draws up the programme, the special requirements of the new technique are not taken into account. In either case the programme is deficient.

How do we escape from this situation? In practice one can find versatile engineers who are able to grasp both sides of the programme. At present there are few such engineers, and so one needs to value them highly. These highly-skilled engineers are just as necessary to us as design engineers, so one needs to encourage them and also give to them the opportunity of playing an active part in production in the capacity of adjusters of new techniques. At Ministries and Central Boards bureaus must be organized with such specialists. It is also to be the task of these bureaus to draw up the required programmes and organise their implementation. It needs to be laid down as a general rule that there should always be a well-developed programme when starting up new production. But here at home little attention is being paid to this important condition.

Finally, the sixth condition concerns the teacher. If he is a scientist, he should be a teacher. It is generally known that success in teaching requires good friendly relations between the teacher and the pupil. Furthermore, any creator of new technique who is passing on his work to industry, whoever it may be—scientist, inventor or collective of a scientific institute—should be interested in its successful adoption the same as the works executive. Are our scientists and our inventors and engineers interested in their achievements being adopted by industry and how are they linked with undertakings and industry?

I shall dwell only on the case where the teacher is a scientist.

As is generally known, a scientist who is engaged on "introduction" in industry, receives no material recompense for this. In our country it is accepted that a scientist should work with industry as a social obligation. This is completely different to the position in capitalist countries. When I lived in Great Britain and received the academic rank of Doctor, as a scientist I joined the professional association of scientific workers and was obliged to sign an undertaking according to which I had no right to serve as a consultant to industry without payment, and what is more there was a definite scale of fees corresponding to my academic rank below which it was forbidden to charge. In capitalist countries this is done so that members of the associations cannot undercut each other. Naturally, at home conditions are quite different and so measures of this kind can have no place here.

I do not want to insist that material recompense in our country is of decisive importance, but undoubtedly conditions must always be favourable for the morale of scientists working with industry. It is necessary for it to be to the interest of the scientist to work, and a milieu needs to be created in which his work would have widespread social recognition and in which collaboration with industry would be considered a useful state activity. Unfortunately, today, whenever one has to turn to a Ministry, rarely are you met with a "kindly" attitude. All this, of course, is not instrumental in developing good relations between teacher and pupils.

Often in our country they assume it is enough to get an order prescribing the adoption of something, to assume that the process of "introduction" is thereby provided for. Yet from the foregoing analysis it follows that the process of adopting new techniques can never be regarded simply as an administrative act; rather this process needs to be approached as one would approach a "pedagogic poem". Hence in organizing the adoption of new techniques by industry, one needs the individual approach, no rigid pattern, and one needs to allow for the nature of people and the conditions prevailing in each particular case. It is by government order, of course, that staffing and financial matters mainly are determined on the spot, but success in adopting new techniques is founded on good relations between the pupil and the teacher and on their common personal interest in success, in implementing their programme, but this in this country unfortunately, is not always the case.

Now I want to touch upon another topic which is no less important: is our science delivering sufficient goods for the economy to adopt, is the productivity of the scientists high enough? To these questions we need to pay serious attention. To look into them, I think the best thing is to compare some data of our scientific work with American data.

It is of interest that today in the U.S.A. they are seriously concerned with the development of science in their country and with its relation to industry, and in this connexion they are publishing much statistical material.*

I quote some figures which are interesting for us. This year the United States of America are spending on all scientific work 21,000 million dollars, of which two thirds comes from the federal budget, and one third from patrons and industry. Of this total academic science receives 11 per cent, or 2500 million dollars, hence the bulk of the expenditure is on science which directly serves for adoption by industry, or, as we say, goes on scientific thematic-applied research.

The Americans further state that with their high cultural level and available capital the existing stock of science is not enough to meet the industrial demand for new techniques. Industry in the U.S.A. mainly needs developments in new directions, such as television and cinema in the past, and now, for instance, cybernetic machines and synthetic fibres. New applications of engineering are not only the most profitable investment, but they also have a social significance since they absorb unemployed workers and they raise the standard of living.

The Americans consider that the inadequate scales of development of theirs are the consequence, mainly, of a shortage of highly-qualified scientists and engineers. They consider that scientific work could produce still more money, but at present they lack highly-gifted people able to direct their own scientific work and so largely determine the development of scientific work in the direction which they need. Therefore in recent years they have started bringing in large numbers of scientists from Great Britain and West Germany. In the last ten years 53,000 scientists have emigrated to America, mainly young scientists, of whom 30,000 had engineering training, 14,000 were physicists, and 9000 scientists in other specialization. This means more than 5000 persons every year.

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* See, for example, *Scientific American*, July, 1965, p. 19.

Assuming that one university produces on an average 500 persons each year, this means that in Europe in the last ten years at least 10 universities have gratuitously trained cadres for America. Insofar as the Americans took the best people, this means that they took the cream of approximately 50 universities. At the Royal Society a special commission was set up to discover the reason for such a large drain of young scientists to America and to decide what measures needed to be taken to stop this exsanguination of British science.

Roughly the following was discovered: although Britons and Germans pay their leading scientists quite well, the Americans pay twice as much as the Britons and Germans. But it appears that this is not the decisive reason for the emigration of scientists. The commission discovered that young scientists were attracted not only by high salaries. The conditions under which scientific work is set up in America attract them.

The Americans allocate their funds to scientific work slightly differently to other countries. The main funds for science are not allocated by scientific institutions. The Americans prefer to allocate their funds to a definite theme, or to support the work of an outstanding scientist, who is usually given the opportunity of a free hand as to the choice of theme. Only a small percentage of budget funds is allocated to scientific institutions (apparently no more than 2–3 per cent). It is of course very attractive to a scientist to be an independent boss of his own material base. They feel that their work can be completely assured, providing great independence and freedom of action is retained.

Let us now contrast American statistical data with ours. Not only is this difficult, it is almost impossible since there is a real difference between the organization of science and its finance in a capitalist and socialist country. The difficulty is still further aggravated by the fact that, unfortunately, we have not as yet properly undertaken the collection of statistics in connexion with the organization of science.

We compare, first of all, the number of scientific workers. The Americans consider that in their country today, starting from the lower posts, from engineers and technicians, there are 800,000 persons occupied in scientific work. In the U.S.S.R. from official statistical material, counting all scientific workers, starting with the juniors, there are about 700,000. Hence it will be seen that within statistical confidence limits we differ little from each other as regards the number of scientific workers. This conclusion is corroborated also by data taken from a French source. They consider that for every 10,000 persons in the population in America there are 23 scientific workers with high qualifications, in the Soviet Union the figure in 18.5 and in Great Britain it is only 9.7, whilst it is much less in all other countries. Since our population is greater than in America, the total number of highly qualified scientific workers in the U.S.S.R. is therefore the same as in the U.S.A.—about 400,000.

To determine the productivity of scientists, one needs to evaluate scientific production. It is, of course, hard to do this precisely. The Americans try to do it in the following way. They have counted the number of scientific papers in the leading branches of natural science and of technical sciences published by scientists in all languages in different countries in the main scientific journals. Judging from this American data we find that today they are doing one third of all world science. We are doing one sixth of all world science, i.e. only half as much as the Americans. Every other country is doing less than we are. Thus as regards scientific production we are the second country in the world. But if we take these figures, we find that with approximately the same number of scientific workers we are only doing half as much scientific work. Therefore, no matter how unfortunate, it has to be recognised that the productivity of our scientists is approximately half that of scientists in the U.S.A. Moreover, as will be seen from further data, recently the rates of growth of science in the U.S.S.R. have started to drop slightly. It is therefore timely to pose the question: how, under the conditions which have been created, are we to develop our science so as to increase the productivity of our scientists?

Productivity in science is determined, firstly, by the material potential at the disposal of the scientist and, secondly, by the quality of education and the selection of cadres in scientific institutions.

Let us consider the first factor. American budget expenditure on science is rapidly growing. Over the last 20 years it has been increasing on an average by 14 per cent annually, and in the present year the growth has reached its greatest yet: 20 per cent. Expenditure from the federal budget alone amounts this year to a sum of 14,000 million dollars.

In the newspaper "Pravda" in the leading article of 17 December 1965 our budget expenditure on science for the coming year was given as 6500 million roubles, an increase of 9.9 per cent over the previous year.

Thus with the same number of scientific workers our material base is much weaker and this, of course, greatly influences productivity. Insofar as today there is no possibility of strengthening the material base, we have no alternative but to reduce the number of new cadres considerably, roughly by half, in order to improve their quality, of course, i.e. it is necessary to sieve out the people who cannot justify by their work the advantages which come to them due to their position as scientist in the country.

Unfortunately, one can see no other possibilities, except in the near future of transferring a whole row of workers who are not working sufficiently effectively in science, into industry where they can be of real benefit to the country. Of course, such a serious step is not being taken at once. But such must be the trend in the development of our scientific institutions. One could, for instance, transfer 10–20 per cent of the cadres each year from our scientific institutions into industry and take in 7.5–10 per cent well selected and trained youg people, thus raising the quality of the cadres without closing the doors to the inflow of fresh forces into science. But here it should be pointed out that even if it is decided to follow such a path, we shall be unable to put such a measure into effect so long as all directors of institutes have the same powers and rights in the Academy of Sciences.

One needs to be unafraid of saying that during the last few years the gap in science between our country and America has not only stopped becoming narrower, it has increased, and we must quickly look for a way of making up for the lost ground. Unless in the near future we increase the productivity of our scientists, unless we improve the conditions for the adoption of the achievements of science and engineering in industry, the problem of catching up with the Americans cannot, of course, be solved. If we decisively and boldly use the advantages which socialist construction gives us in the organization of our science and industry, then this lag in growth will only be a temporary hitch. I sincerely believe that if we are not afraid of speaking the truth about our shortcomings and errors and if we amicably look for ways of eliminating them, we shall soon again be leading in the growth of our scientific work.

It is necessary gradually and continuously to improve the organizational forms for the development of our science: improve the material base, raise the quality of cadres and increase the productivity of the scientist. We, as members of the Academy of Sciences of the U.S.S.R., need to apply ourselves especially seriously to these matters. We are the leading scientific institution in the country and therefore we more than anybody else are responsible for the development of science and the adoption of its achievements.

31. RECOLLECTIONS OF LORD RUTHERFORD

I am greatly honoured by the invitation of the President of the Royal Society to speak to you on my reminiscences of Lord Rutherford. But this is a very difficult task and I accepted it after long deliberation. At first sight I thought that to speak about the scientific achievements of so great a scientist as Rutherford would be easy. The greater the achievements of a scientist the more exactly and briefly can they be described. Rutherford created the modern study of radioactivity; he was the first to understand that it is the spontaneous disintegration of the atoms of radioactive elements. He was the first to produce the artificial disintegration of the nucleus and finally he was the first to discover that the atom has a planetary system. Each of these achievements is sufficient to make a man a great physicist. But nowadays these achievements and their fundamental values are well known not only to research students but even to schoolboys. Equally we all know the very simple and beautiful classical experiments by means of which Rutherford made his great discoveries. To come from the Soviet Union to speak about all this to the Fellows of the Royal Society would scarcely be appropriate.

You are all well aware that from research into radioactivity there grew up an independent science which is now called nuclear physics, and of all the papers published on physical research one fifth relate to the investigation of nuclear phenomena. Both nuclear energy and the use of artificial radioactivity in science and technology are developing quickly and simultaneously. All these fields absorb the main bulk of the monetary resources spent on science and which now reaches the sum of thousands of millions of pounds, dollars and roubles. And all this for the last 30 years grew out of one modest domain of physics which in the old days was called radioactivity and the father of which is justly called Rutherford. To speak of the development of nuclear technology and physics which came from the work of Rutherford and his school is very interesting and very instructive. But I am sure that such Fellows of the Royal Society as our President Professor Blackett, Sir James Chadwick, Sir John Cockcroft, Sir Charles Ellis and Sir Mark Oliphant who in the old days were the most active members of Rutherford's school and who themselves in this domain have made fundamental discoveries and researches are certainly more qualified than I to speak on these matters.

The only way in which I can satisfy the interest of the Fellows of the Royal Society is to speak of Rutherford the man, of how I remember him during my 13 years' work in the Cavendish Laboratory, of how he worked, how he trained

P. L. Kapitza, Recollections of Lord Rutherford, *Proc. Roy. Soc.* A, 294, 123 (1966); text of a lecture delivered before the Royal Society on 17 May 1966.

us young scientists and also of his relations with the scientific world. My task is therefore to draw you a portrait of the great scientist and of a great man. Frankly, this is the job of a writer and not of a scientist. If I have now decided to do so, this is mainly for the following reasons. When I look back and see myself as a young man coming to England in 1921 and starting work in the Cavendish Laboratory and, after 13 years, growing into a scientist, I feel that these years of my work were the happiest, and for all that I have been able to achieve I feel immensely grateful for the attention and kindness which Rutherford showed me, not only as a teacher but as a very kind and sympathetic man for whom I have a sincere affection and with whom I eventually became great friends.

However imperfect my recollections of Rutherford may be, this is the only way in which I can express my deep gratitude to this great and remarkable man.

As is well known, Rutherford was not only a great scientist but also a great teacher. I can recall no other scientist contemporary to Rutherford in whose laboratory so many outstanding physicists were trained. The history of science tells us that an outstanding scientist is not necessarily a great man, but a great teacher must be a great man. Therefore my task is even more difficult: I must give you a portrait not only of a scientist but of a man. I will attempt to make my portrait of Rutherford as alive as possible and for this purpose I shall illustrate my talk with episodes which I most vividly remember. From my many recollections I shall select the ones which characterize different sides of Rutherford's nature. I hope this will help you to reconstruct a lifelike picture of Rutherford in your imagination from all these fragments.

I would like to begin my recollections with a small episode which happened in 1930 in the Cavendish Laboratory. At that time a small conference was being held in Cambridge to commemorate the centenary of the birth of Maxwell, the first director of the Cavendish Laboratory. He was succeeded by Rayleigh, J. J. Thomson and Rutherford, four great physicists of the last and present century. After the official part of the meeting in which some of Maxwell's pupils talked of their reminiscences, Rutherford asked me how I liked the speeches. I answered that they were very interesting, but I was surprised that all the speakers spoke only of the positive side of Maxwell's work and personality and made a 'sugary extract' (*sic*) of him and I said that I would like to see Maxwell presented as a living figure with all his human traits and faults which of course every man possesses however great his genius. Rutherford as usual laughed and said that he charged me after his death to tell future generations what he was really like. Rutherford was joking and I was laughing too. And now when I try to fulfil his behest and I imagine Rutherford as I have to present him before you, I see that time has absorbed all his minor human imperfections and I can only see a great man with an astounding brain and great human qualities. How well I now understand Maxwell's pupils who spoke about him in Cambridge.

There are numerous books and articles on Rutherford as a scientist. It is widely recognized that the simplicity and clarity of his thinking, his great intuition and great temperament were very characteristic of his creative ability.

Studying the works of Rutherford and observing how we worked I think the basic characteristics of his thinking were great independence and hence great daring.

The basic method by which science is developing consists of experimental investigations into natural phenomena and the continuous verification of the consistency of the results of our investigations with our theoretical conceptions. The progress of our knowledge of nature appears in cases when we find contradictions between theory and observation, and these contradictions, as they compel us to develop our theories, enable us to widen our knowledge of nature. The more acute these contradictions are, the more they lead us to further fundamental changes in understanding the laws of nature, on the basis of which we may use nature for our cultural development. In science, as in history, definite stages of development demand their particular kind of genius. A definite period of development requires men with corresponding mental abilities. In the history of the development of physics, as in any other experimental science, the most interesting periods are those in which we are brought to revise our fundamental scientific conceptions. Then not only deep thinking and intuition are required from the scientist but also a daring imagination. As an illustration I shall remind you of two well known cases in the history of physics. They made a great impression on me personally. The first case concerns Franklin's creation of the study of electricity. On the basis of this study Franklin stated that electricity has a material origin and can impregnate metal and freely move in it. In his day this concept was in fundamental contradiction to the concept of the continuous nature of matter. But Franklin's view was eventually accepted as it gave a simple and complete explanation of all the electrostatic phenomena observed in his day. It is only recently, 150 years later, after J. J. Thomson discovered the electron, that Franklin's concepts were completely justified. But the most striking thing in this story is how it could have happened that Franklin, who had never before done any scientific work, could, in the course of a few years in a small remote American town when he was already a middle-aged man, find the right way by which this most important branch of science should be developed. And this happened in the middle of the eighteenth century when science was developing on the level of Newton, Huygens and Euler. How could Franklin achieve such results which were beyond the reach of professional scientists?

The other similar case in which the fundamental concepts of electricity had to be revised in the light of experiments is also well known. This is Faraday's concept on the electrical field. It is difficult to find a more revolutionary and original idea than Faraday's. He advanced the concept that electrodynamical processes must be explained by the phenomena happening in the space surrounding the conductor. I mention this case mainly because Faraday was a scientist who had no traditional scientific education, even though at that time its level was high for an average English scientist. I mention these two well known cases only to show that at a particular stage of the development of science, when new fundamental concepts have to be found, wide erudition and conventional training are not the most important characteristics of a scientist required to solve this kind of problem. It appears that in this case imagination, very concrete thinking and, most of all, daring are needed. Strict logical thinking which is so necessary in mathematics hinders the imagination of a scientist when new fundamental concepts must be found. The ability of a scientist to solve such scientific problems without showing a logical trend of thought is usually called 'intuition'. Possibly there is a way of thinking which takes place in our subconscious but the laws by which it is governed are at present unknown. If I am not mistaken, even Freud, a pioneer in the study of subconscious processes, was not aware of it. But if intuition exists as a powerful creative thinking process then doubtless Franklin and Faraday mastered it thoroughly. I am sure that Rutherford mastered it too and he has rightly been called the Faraday of our time.

When at the beginning of our century Rutherford started studying radioactivity it had already been proved experimentally that these phenomena contradicted the most fundamental law of nature, the law of conservation of energy. The explanation of radioactivity which Rutherford gave, namely the disintegration of matter, at once provided not only the key to the understanding of these phenomena but also led all investigation in the right direction. The same thing happened when Rutherford created the planetary model of the atom. At first sight this model completely contradicted laws of classical electrodynamics since in its circular motion an electron was perpetually bound to lose by radiation its kinetic energy. But the experiments of scattering the α-particles, performed by Rutherford's pupil Marsden in 1910, definitely showed the existence of a heavy nucleus in the centre of the atom. Rutherford imagined the collision of particles so clearly that even these contradictions with the fundamental laws of electrodynamics could not prevent him from establishing the planetary structure of the atom. We know that only three years later Bohr, on the basis of the developing quantum theory of light, evolved his brilliant theory of the structure of the atom which not only justified Rutherford's planetary model but also quantitatively explained the spectra of atomic radiation.

The peculiar character of Rutherford's thinking could easily be followed when talking to him on scientific topics. He liked being told about new experiments, but you could easily and immediately see by his expression whether he was listening with interest or whether he was bored. You had to talk only about fundamental facts and ideas without going into the technical details in which Rutherford took no interest. I remember, when I had to bring him for approval my drawings of the impulse generator for strong magnetic fields, for politeness sake he would put them on the table before him, without noticing that they were lying upside down, and he would say to me: 'These blueprints don't interest me. Please state simply the principle on which this machine works.' He grasped the basic idea of an experiment extremely quickly, in half a word. This struck me very much, especially during my first years in Cambridge when my knowledge of English was poor and I spoke it so badly that I could only vaguely explain my ideas. In spite of this Rutherford caught on very quickly and always expressed very interesting opinions.

Rutherford also liked talking about his own experiments. When he was explaining something he usually made drawings. For this purpose he kept small bits of pencil in his waistcoat pocket. He held them in a peculiar way—it always seemed to me a very inconvenient one—with the tips of his fingers and thumb. He drew with a slightly shaky hand, his drawings were always simple and consisted of a few thickly drawn lines, made by pressing hard on the pencil. More often than not the point of the pencil broke and then he would take another bit from his pocket.

A number of physicists, especially theoreticians, like to discuss science and apparently the process of argument is a way of thinking. I never heard Rutherford argue about science. Usually he gave his views on the subject very briefly, with the maximum of clarity and very directly. If anybody contradicted him he listened to the argument with interest but would not answer it and then the discussion ended.

I greatly enjoyed Rutherford's lectures. I followed the course of general physics which he gave to the undergraduates as Cavendish Professor. I did not learn much physics from this course since by that time I already possessed a fair knowledge of the subject, but from Rutherford's approach to it I learnt a great deal. Rutherford delivered his lectures with great enthusiasm. He used hardly any mathematical formulae, he used diagrams widely and accompanied his lectures with very precise but restrained gestures from which it could be seen how vividly and picturesquely Rutherford thought. I found it interesting that during the lecture he changed the topic as his thoughts, probably following some analogy, turned to a different phenomenon. This was usually connected with some new experiments made in the field of radioactivity which fascinated him and he then proceeded to speak with enthusiasm on the new subject. In this case he usually put his assistant in a difficult position by asking him to give a demonstration which was not part of the original planned version.

About the same time I also attended the lectures of J. J. Thomson in his special course on the conductivity of electricity through gas. It was interesting to notice how differently these two great scientists approached scientific problems. If Rutherford's way of thinking was inductive, then the way of thinking of J. J. Thomson was deductive.

I think it useful when training young scientists to ask them to follow a course of lectures, even an elementary one, but delivered by an eminent scientist. Listening to these lectures they will learn something that they will never find in any textbooks. In this connexion I remember a conversation which I had with Sir Horace Lamb. He was telling me how he had attended Maxwell's lectures. Maxwell, he said, was not a brilliant lecturer; he usually came to lectures without any notes. When he was doing mathematics on the blackboard he often made mistakes and sometimes got muddled. From the way in which Maxwell tried to disentangle and correct his mistakes Lamb learned more than from any textbooks he ever read. Lamb told me that for him the most precious parts of Maxwell's lectures were those in which he made mistakes. No doubt the mistakes of a genius are sometimes as instructive as his achievements.

When I came to Cambridge Rutherford did no more experimental work by himself; he worked chiefly either with Chadwick or with Ellis. But in both cases he took an active part in experiments. The setting up of the apparatus was done mainly by his laboratory assistant, Crow, whom he treated rather severely. But I sometimes saw how Rutherford himself, despite his slightly shaking hands, dealt quite skilfully with the finewalled glass tubes filled with radium emanation. Although Rutherford's experiments are well known, I cannot refrain from saying a few words about them. The most attractive thing about these experiments was the clarity of setting the problem. The simplicity and directness of approach to the solution of the problem were most remarkable. From my long experience as an experimenter I have learned that the best way of correctly evaluating the capacity of a beginner as well as of a mature scientist is by his natural inclination and ability to find a simple way of solving problems. There is an excellent saying by an unknown French author which applies perfectly to Rutherford: 'La simplicité c'est la plus grande sagesse.' I should also like to quote the profound saying of a Ukrainian philosopher, Gregory Skovoroda. He was by origin a peasant and lived in the second half of the eighteenth century. His writings are most interesting but probably quite unknown in England. He said 'We must be grateful to God that He created the world in such a way that everything simple is true and everything complicated is untrue.' Rutherford's finest and simplest experiments concerned the phenomena of scattering by nuclear collisions. The methods of observation of scintillations by counters were worked out by Rutherford in collaboration with Geiger in 1908. Since then more than half a century has passed and this method and the Wilson chamber invented about the same time remain the fundamental methods for studying nuclear phenomena, and only the optical and resonance methods for determining nuclear moments have since been added. And up to now all nuclear physics possesses no experimental possibilities other than those used by Rutherford and his collaborators. The present development of nuclear physics is proceeding not by the invention of new experimental possibilities of investigating nuclear phenomena but thanks to the possibility of investigating nuclear collisions of a *larger* number of elements; and these collisions are studied in the domain of larger energies which are reached mainly by the use of powerful modern accelerators. But even now the way which leads us to the knowledge of the nucleus is still the method discovered by Rutherford, and he was the first to appreciate its fundamental value. I am referring here to the investigation of the collision of nuclei. Rutherford always liked to say 'Smash the atom!'

Even now, in the process of investigating nuclear collisions, there is one great weakness: the necessity of using statistical methods in the interpretation of experimental results. Great care is required to deduce correct general laws from limited statistical data. Someone once said about statistics: 'There are three kinds of lies: a simple lie, an impudent lie and statistics.' In fact this was said about the application of statistics to social problems. But to some extent it is true of statistics in physics. I do not think that in any other branch of physics so many mistakes and faulty discoveries were made as in the course of the interpretation of statistical data obtained from experiments on nuclear

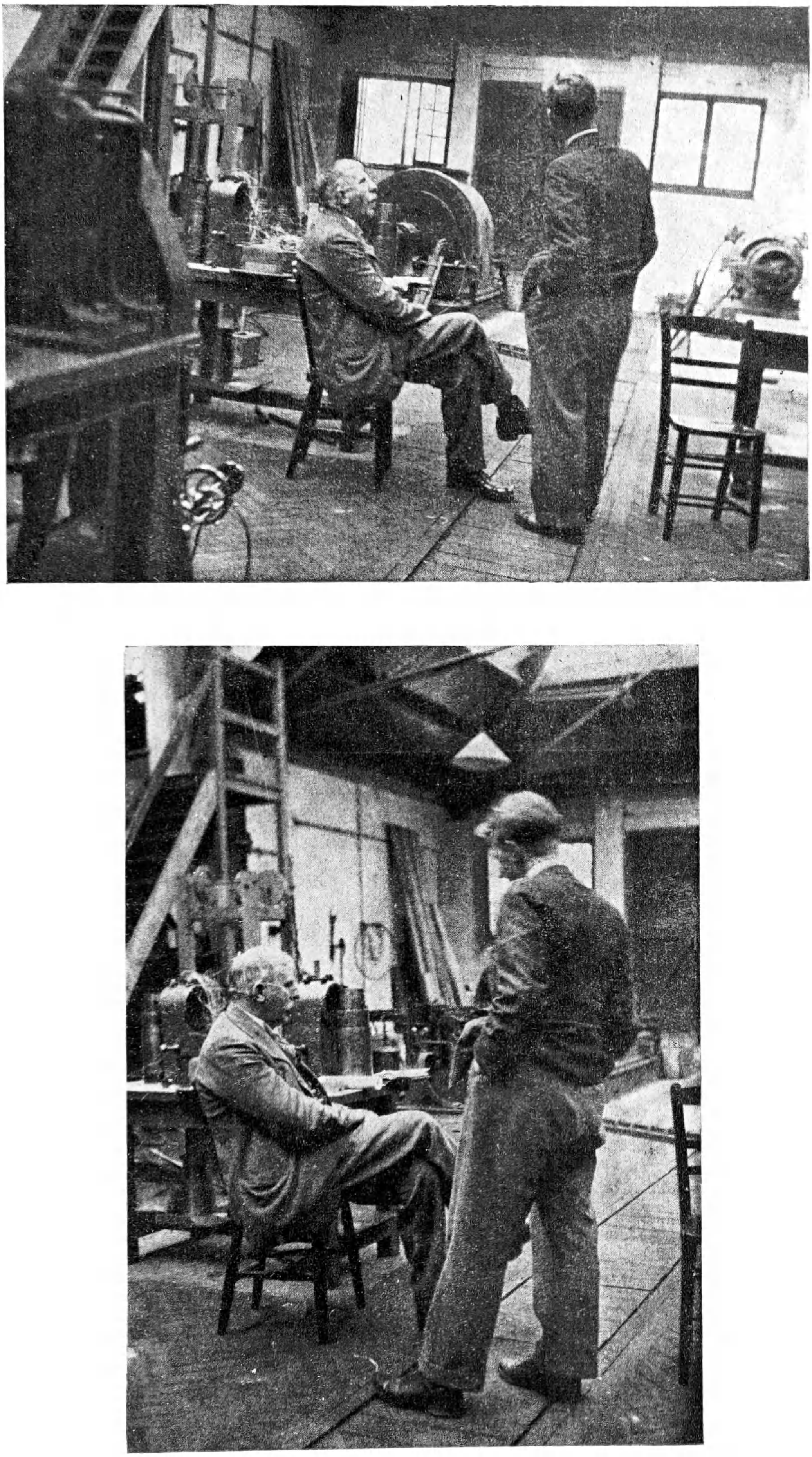

Plate 25. Two views of Rutherford and Cockcroft standing in front of the high power impulse generator for producing high magnetic fields, taken about 1930.

PLATE 26a. Rutherford in the Cavendish Laboratory for high magnetic fields.

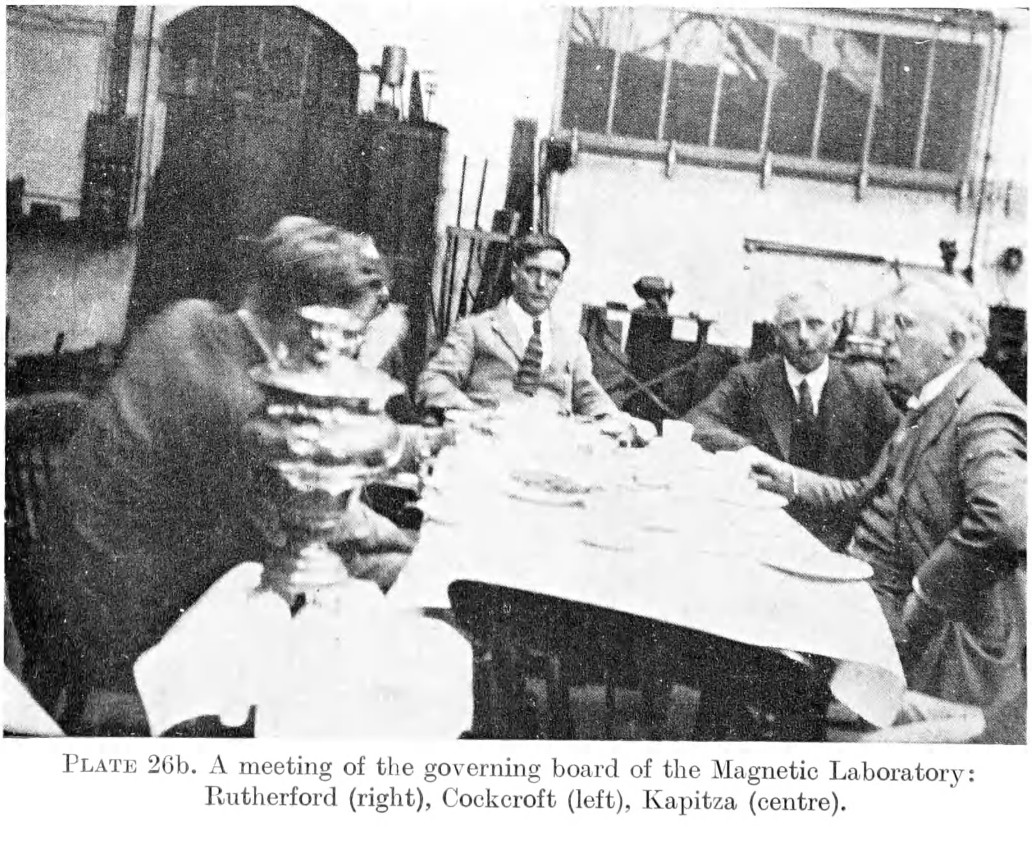

PLATE 26b. A meeting of the governing board of the Magnetic Laboratory: Rutherford (right), Cockcroft (left), Kapitza (centre).

collisions. Nearly every year new particles of resonance levels are still discovered, some of which may not exist. Rutherford was well aware what danger lies concealed in the interpretation of experimental data of statistical origin, especially when the scientist anticipates definite results. Therefore Rutherford was very careful to exclude the personal element and took the following precautions during the course of these experiments: the counting of scintillations was usually done by undergraduates who did not know the purpose of the experiment; the curves were drawn by persons who did not know what results were expected. As far as I remember, Rutherford and his pupils never made a single such mistake, while in the same line of investigation a number of mistakes were made in other laboratories. I remember that in those days the most critical approach in the interpretation of statistical data was that of Chadwick on whose judgement Rutherford usually relied completely.

I did not work with Rutherford because my investigations were not connected with nuclear physics and therefore I did not see him working in his laboratory. But I know that up to the every end of his life the main bulk of his time was taken up by his personal scientific research. I expect he gave the same amount of attention and strength to directing the work of young research pupils working in the Cavendish Laboratory. The detailed guidance of scientific work he left to one of the senior scientific workers, usually Chadwick. But he himself always took an interest in the choice of problem for experiment and of the experimental approach. Until the research student began obtaining results Rutherford showed no marked interest in his work. He never bothered about detailed guidance.

He often came to the laboratory but only for a short time; just to make remarks like: 'Why don't you get a move on—when are you going to get some results?' When I started working in the Cavendish Laboratory such remarks made a great impression on me, especially as they were made in a thundering voice and with a severe expression. But eventually I found out that such utterances were automatic, maybe customary for a New Zealand farmer who when going through the fields found it useful to stimulate the workers with a few 'kind' words. That it was actually so was proved by an episode which happened a few years later in the Cavendish. One day it was necessary to break a hole through a stone wall to put through a cable needed for some experiment. The work was urgent and it happened that at that time there was a building strike and it was exceptionally difficult to find a bricklayer who would consent to work. Finally a man was found and he started work, but after a while he came and said that he refused to go on. When asked why, he replied that twice a gentleman had passed by him and both times had asked him when he would finally start work and get the job done. These remarks offended the workman. When asked who this gentleman was, his description showed without a doubt that it was Rutherford. When we reproachfully pointed out to Rutherford that during a strike one should be a little careful we were surprised that Rutherford denied having said anything to the bricklayer. Obviously when he likewise grumbled at us in the laboratory for our slow work, he did it unconsciously; it was a kind of conditional reflex.

The greatest quality of Rutherford as a teacher was his ability to direct research work in the right direction, then to encourage the beginner and to give just appraisal of his achievements. What he valued most in a pupil was independent thought and originality in his work. Rutherford did his utmost to develop in his pupil an individuality. I remember how in the first years of my work in the Cavendish I once said to Rutherford: 'You know that the work of X is pretty hopeless; don't you think he's wasting his time and apparatus?' Rutherford replied that he too knew that the man was working on a hopeless problem, 'but', said Rutherford, 'it is a problem of his own and even if the work cannot be accomplished it will lead him to another original research problem which will be successful'. The future showed that Rutherford was right. As I said, Rutherford would do his utmost to develop in his pupils independence and originality of thought and as soon as a pupil showed these qualities Rutherford would pay close attention to his work. As an example of Rutherford's ability to direct the research of his pupils I remember the story, as Rutherford told it himself, of the discovery made by Moseley. In 1912 Moseley worked with Rutherford in Manchester. He was very young and Rutherford spoke of him as one of his best pupils. When Moseley came to Manchester he at once accomplished some minor research work and then eventually he came to Rutherford and told him of three different topics he would like to investigate. One of these researches was the classical work which had made Moseley's name so well known—the dependence of the wavelengths of Roentgen rays on the position of atoms in the periodical system. Rutherford at once advised Moseley to choose this work for his investigation. The future showed that Rutherford made the right choice, but he always pointed out that the idea of the experiment belonged to Moseley.

Rutherford was very particular to give credit for the exact authorship of any idea. He always did this in his lectures as well as in his published works. If anybody in the laboratory forgot to mention the author of the idea Rutherford always corrected him. He was also very particular not to give a beginner technically difficult research work. He reckoned that, even if a man was able, he needed some success to begin with. Otherwise he might be disappointed in his abilities, which could be disastrous for his future. Any success of a young research worker must be duly appreciated and must be duly acknowledged.

Once, in one of our outspoken talks, he told me that the most important thing a teacher must learn is not to be jealous of the successes of his pupils—which is not so easily done as the teacher gets older! This profound truth made a great impression on me. No doubt the greatest quality of a good teacher should be generosity. Rutherford was undoubtedly very generous and I think this is one of the main secrets which explains why so many first-class scientists came from his laboratory. There was always an atmosphere of freedom and efficiency there.

Rutherford well understood the importance that his pupils had for him. It was not merely that young research students increased the scientific productivity of the laboratory, but, as he said, 'My pupils keep me young'. This is very true, since pupils do not permit a teacher to lag behind new achievements in science. How often do we notice that when a scientist is ageing he starts opposing new ideas and underestimates the significance of new trends in science. Rutherford, with great ease and generosity, always accepted new ideas in physics like wave quantum mechanics, while a number of distinguished scientists of his generation were sceptical of the same ideas. Such conservatism is characteristic of scientists who work by themselves without having pupils to be directed and encouraged.

Rutherford was very sociable and loved talking to the scientists who came to visit him and the Cavendish Laboratory. Usually there were many such visitors. His attitude to other people's work was kind and considerate. In conversation Rutherford was very lively; he was fond of jokes and often made them himself. He laughed easily, his laughter was sincere, loud and infectious. His face was very expressive: you could see at once what mood he was in, good or bad, or whenever he was worried by anything. You always knew he was in a good temper when he good-naturedly teased the person he was talking to. The more he teased him, the more he liked him. This was particularly noticeable when he talked to Bohr or to Langevin to both of whom he was especially attached. His kindest jokes often concealed a deeper sense. I remember one occasion when he brought Professor Robert Millikan to my room in the laboratory. Rutherford said to me, 'Let me introduce you to Millikan; no doubt you know who he is. Show him your installation to produce strong magnetic fields and tell him about your experiments. But I doubt whether he will let you speak as he himself will tell you about his own experiments!' There followed loud laughter in which Millikan joined with rather less enthusiasm. Rutherford then left us, and I soon found out that his prophecy was correct.

I shall not describe the way in which Rutherford read his papers. I always liked them very much as regards both their content and their exposition. He attached great importance to the way in which his papers were presented and evidently prepared them very carefully. He taught me how to read papers to the Royal Society, and one of his instructions I still remember very clearly: 'Don't show too many slides. When it is dark in the lecture room some of the audience take the opportunity to leave!'

Rutherford's interests were not limited narrowly to physics; they were much wider. He was well read, he liked books on geography and history and liked to discuss what he had read. He absorbed all knowledge enthusiastically and always extracted the essentials.

Later on, when I became a Fellow of Trinity and used to accompany him home after dinner on Sundays, we often discussed politics. On the first day I started work in the Cavendish I was surprised to hear him saying to me that in no circumstances would he tolerate my making Communist propaganda in his laboratory. At this time this remark came quite unexpectedly. It not only surprised me, but also shocked me and to a certain extent even offended me. Undoubtedly it was a consequence of the current atmosphere of acute political struggle and was connected with the propaganda which existed in those days, only four years after the Russian revolution. Before coming to England, I was so absorbed by my research work in Russia that I was completely unaware of what was happening in Western Europe and could not appreciate the scale of the bitter political controversy which then existed. Later on when my first experimental research was published I presented Rutherford with a reprint and I made an inscription on it that this work was proof that I had come to his laboratory to do scientific work and not to make Communist propaganda. He got extremely angry with this inscription, swore and gave me the reprint back. I had foreseen this and I had another reprint in reserve with an extremely appropriate inscription with which I immediately presented him. Obviously Rutherford appreciated my foresight and the incident closed. Rutherford had a characteristically hot temper but cooled down just as quickly.

Eventually we had many conversatious on political questions; we were especially concerned about the growth of fascism in Europe. Rutherford was an optimist and thought that all would soon be over. We now know that this was not the case. Rutherford, like most scientists who work in the exact sciences, had progressive political views. I involved Rutherford in some political activity on two occasions.

The first of these was connected with Langevin. In his younger days Rutherford had worked with Langevin in the same room at the Cavendish. A deep friendship developed between them. Indeed it was practically impossible not to be friendly with a man of such brilliant intelligence and exceptional moral qualities. In Paris my friends, pupils of Langevin, were greatly shocked that Langevin, undoubtedly the best French physicist, had not been elected to the French Academy as a result of his left wing political views. Langevin had taken part in a number of progressive organizations, had been the founder of the League of the Rights of Man (*ligue des droits d'hommes*) and had fought anti-semitism in the Dreyfus case. I told Rutherford of the difficulties Langevin had encountered in France and asked him whether a man who held such leftist views as Langevin could be a Foreign Member of the Royal Society. Rutherford said something I could not quite follow, then started to tell me what a really good man Langevin was, and then recalled that during the war Langevin had been very active in inventing supersonic beams propagated in water by which he had established communication between England and France across the Channel. At this point the conservation ended. I learnt later that at the next election in 1928 Langevin was elected a Foreign Member of the Royal Society and this was much earlier than his election to the French Academy.

The second example occurred much later, when Hitler started to come into power. We were very anxious about the fate of such distinguished physicists as Stern, Frank, Born and a number of others in the conditions of active and increasing anti-semitism in Germany. About this time Szillard came to England and we were faced with the question of how to get these scientists out of Germany without raising suspicion. I spoke to Rutherford and he was very willing to help, writing personal letters to these scientists, and inviting them to come to lecture in Cambridge.

Rutherford took an interest in a great variety of people, but he particularly iked people with strong personalities. When Rutherford was elected President of the Royal Society and often had to attend dinner parties with distinguished politicians, businessmen and statesmen, he was fond of telling stories afterwards about the conversations he had had with them and always gave descriptions of them. I specially remember that Churchill made a great impression on Rutherford. His description of Churchill was, like all his descriptions, short and clear, and in due course I found out that it was quite correct. I well remember that Churchill in those days already regarded Hitler as a real danger to peace and called him 'a man riding a tiger'. Possibly this conversation somewhat altered Rutherford's optimistic view of the future. Rutherford's interest in understanding human psychology and his kindness to others was undoubtedly felt by them. This explains why Rutherford's excessively direct way of speaking, which was sometimes not very tactful, was completely compensated for by his kindness and cordiality.

Of course Rutherford's correct evaluation of people and his understanding of them was due to the fact that he was a subtle psychologist. People interested him and he had the faculty of understanding them. His assessments of people were always very outspoken and direct. As in his scientific work, his description of a man was always brief and very accurate. I was always convinced that his descriptions were correct. Possibly his approach to people was also a subconscious process and could be called intuitive.

I should like to illustrate his interest in psychology with the following two episodes. In Cambridge there was a small but progressive theatre which produced Chekov's play 'Uncle Vanya'. Rutherford went to this play and was greatly taken with it. As in all Chekov's works, it deals with a psychological problem complicated by the fact that all the people in the play are highly intellectual and therefore their acceptance of life is very complex. In the play a certain retired professor comes to live on the estate of his wife. Uncle Vanya, who manages the estate, has devoted his whole life to supporting the professor. Soon Uncle Vanya finds that he professor is a fake celebrity, scholastic and pedantic in his work. Against a background of complex psychological situations Uncle Vanya fires a pistol at the professor but misses him. I remember how vividly, clearly and simply Rutherford told me this plot and his sympathy was completely on Uncle Vanya's side. The fact that Rutherford was so attracted by the play shows that he undoubtedly enjoyed disentangling complicated psychological cases of this kind.

A great impression was made on me by the following case which demonstrates Rutherford's skill in handling complicated psychological problems. I think enough time has now passed and I can tell you about this case which involved the then well known physicist, Paul Ehrenfest.

Ehrenfest was born in Austria. On one of his mountaineering excursions he met a Russian woman scientist and followed her to Russia where he married her. In Russia he published a number of outstanding theoretical works on thermodynamics. Eventually he was invited to Leiden University to take the chair of theoretical physics vacated by the great Lorentz, creator of the electronic theory of metals and one of the founders of the theory of relativity. In Leiden Ehrenfest and his house became one of the world centres of theoretical physics. Ehrenfest's main quality was his precise critical mind. He was not only a very good teacher of young scientists, who were very fond of him, but his criticisms were regarded as profound and of such high quality that leading theoretical scientists like Einstein and Bohr often came to Ehrenfest to discuss their work. Ehrenfest always noticed even the smallest contradiction or mistake. His critical remarks were made very readily, with great spirit and even sharply, but always very goodnaturedly. The quality of his criticism was greatly appreciated. Despite our difference in age we became friends and I often visited his very hospitable and very charming family and more than once was present at his scientific discussions.

Ehrenfest's exceptionally strong critical mind evidently acted as a restraint on his creative imagination and he did not succeed in producing scientific work which he himself would have considered of sufficiently high standard. In those days I did not know that in his acute nervous condition Ehrenfest suffered greatly when he could not in his work attain the level of the friends he criticized. I learnt about his feelings in the following manner. In the beginning of 1933 I received a long letter from him in which he described in detail his state of mental depression and spoke of the futility of his achievements. He had come to the conclusion that it was not worth living any more. The only way to save himself, he thought, was to leave Leiden and settle somewhere away from his friends. He asked me to help him to find a chair at some small university in Canada and to ask Rutherford, who doubtless had connexions in Canada, to assist him. I was, of course, very upset by this letter. We all liked Ehrenfest and all knew that his influence as a teacher and critic in the development of modern physics was colossal. I translated the letter from German into English and came to Rutherford, who had little personal acquaintance with Ehrenfest. I handed Rutherford the letter and told him that we were very worried about Ehrenfest's future as, without any doubt, the letter showed that he was mentally unbalanced; perhaps, I said, this state was only temporary and everything possible should be done to help him out of his state of depression. Rutherford said I must not worry and he would handle the case himself. I do not know what Rutherford wrote to Ehrenfest but shortly afterwards I received a letter telling me that he was once again in a happy frame of mind. He said that Rutherford had explained what a great role he was playing in physics and he added that of course there was now no need for him to go to Canada. This story shows how skilfully Rutherford dealt with a very complicated psychological case, probably better than a professional psychiatrist.

A few months later, while I was on a visit to Russia, the state of depression returned to Ehrenfest and on 25 September 1933 he committed suicide.

I should now like to recall quite a different and rather amusing case characteristic of Rutherford's attitude to the young. Once Rutherford called me into his study and I found him reading a letter and roaring with laughter. It appeared that the letter was from some Ukrainian schoolboys. They has written to say that they had organized a physics club and were proposing to continue Rutherford's fundamental work on the study of the nucleus of the atom and ask him to be an honorary member and to send them reprints of his scientific work.

In the part of the letter in which they described Rutherford's achievements in nuclear physics, instead of using the correct term in physics they used a corresponding term which in slang has a different meaning. In this way the description of the structure of the atom acquired a property of the living organism. Its character is such that one does not speak about it in polite society, and it made Rutherford laugh heartily. I explained to Rutherford that the schoolboys were apparently not very well versed in English and the writing of the letter was mostly done with the use of a dictionary and the mistake was bona fide. Rutherford said that he appreciated this. He sent the boys a reply, thanking them for the honour of being elected a member of the club and promising to send them reprints.

Finally, I should like to discuss a question I have come across several times in descriptions of Rutherford's activities. The question is: did Rutherford foresee the great practical consequences which would emerge from his scientific discoveries and investigations into radioactivity?

The immense reserves of energy which are hidden in matter was understood by physicists a long time ago. The development of his view took place side by side with the development of the theory of relativity. The question which was not clear at that time was: would it eventually be possible to find technical means of making practical use of these reserves? We know now that the actual possibility of obtaining energy from nuclear collisions was becoming more and more real as nuclear phenomena were better understood. But up to the last moment it was not certain whether it would be technically possible to produce nuclear reactions with a great yield of energy. I remember only rare occasions on which I discussed this question with Rutherford and in all these conversations he expressed no interest in it. From the beginning of my acquaintance with Rutherford I noticed that he never took any interest in technical problems and I even had the impression that he was prejudiced about applied problems. Possibly this was because such problems were connected with business interests.

I am by training a chartered engineer and naturally I always took an interest in solving technical problems. During my stay in Cambridge I was approached several times to help in solving technical problems in industry. In these cases I used to take advice from Rutherford and he always said to me: 'You cannot serve God and Mammon at the same time.' Of course he was right. Once I remember Rutherford telling me about Pupin who as an able young physicist had come to Cambridge and done successful scientific work in the Cavendish Laboratory. Pupin was somewhat senior to Rutherford so they met only occasionally. Eventually Pupin turned to commercial activity in the U.S.A. and made a lot of money. Rutherford spoke disapprovingly of Pupin's activities. So I think that Rutherford's opinions on the practical applications of nuclear physics had no real value as they lay outside the scope of his interests and tastes.

In connexion with Rutherford's views on industry I remember a conversation I had with him during a high table dinner at Trinity College. I do not remember how the conversation started, maybe it was under the influence of Lombroso's book, *Genius and Madness*. I was telling my neighbour that every great scientist must be to some extent a madman. Rutherford overheard this conversation and asked me, 'In your opinion, Kapitza, am I mad too?'

'Yes, Professor', I replied.

'How will you prove it?' he asked.

'Very simply', I replied. 'Maybe you remember a few days ago you mentioned to me that you had had a letter from the U.S.A., from a big American company. (I do not remember now which one it was, possibly General Electric Co.) In this letter they offered to build you a colossal laboratory in America and to pay you a fabulous salary. You only laughed at the offer and would not consider it seriously. I think you will agree with me that from the point of view of an ordinary man you acted like a madman!' Rutherford laughed and said that in all probability I was right.

The last time I saw Rutherford was in the autumn of 1934 when I went as usual to the Soviet Union to see my mother and my friends and unexpectedly was deprived of the possibility of returning to Cambridge. I did not hear his voice again, nor hear him laugh. For the next three years I had no laboratory to work in and was unable to continue my scientific work and the only scientist with whom I freely corresponded outside Russia was Rutherford. At least once every two months he wrote me long letters which I greatly valued. In these letters he gave me an account or life in Cambridge, spoke about the scientific achievements of himself and his pupils, wrote about himself, made jokes, gave good advice and invariably cheered me up in my difficult position. He understood that the important thing for me was to start my scientific work which had been interrupted for several years. It is no secret it was only due to his intervention and help that I was able to obtain the scientific installation and apparatus of the Mond Laboratory and in three years time I was able to renew my work in the domain of low-temperature physics.

I am sure that in the course of time all Rutherford's letters will be published, but even so I should like here and now to quote three short extracts which require no comment.

On 21 November 1935 he wrote:

'...I am inclined to give you a little advice, even though it may not be necessary. I think it will be important for you to get down to work on the installation of the laboratory as soon as possible, and try to train your assistants to be useful. I think you will find many of your troubles will fall from you when you are hard at work again, and I am confident that your relations with the authorities will improve at once when they see that you are working wholeheartedly to get your show going. I would not worry too much about the attitude or opinions of individuals, provided they do not interfere with your work. I daresay you will think I do not understand the situation, but I am sure that chances of your happiness in the future depend on your keeping your nose down to the grindstone in the laboratory. Too much introspection is bad for anybody!...'

On 15 May 1936 he wrote:

'... This term I have been busier than I have ever been, but as you know my temper has improved during recent years, and I am not aware that anyone has suffered from it for the last few weeks!...

'... Get down to some research even though it may not be of an epoch-making kind as soon as you can and you will feel happier. The harder the work the less time you will have for other troubles. As you know, "a reasonable number of fleas is good for a dog"—but I expect you feel you have more than the average number!'

You see what short and clear and fatherly advice he gave me. The last letter is dated 9 October 1937. He wrote in great detail about his proposed journey to India. In the last part of the letter he said: '... I am glad to say that I am feeling physically pretty fit, but I wish that life was not quite so strenuous in term time. ...' Ten days before his death he did not feel that it was so near.

For me the death of Rutherford meant not only the loss of a great teacher and friend; for me, as for a number of scientists, it was also the end of a whole epoch in science.

Obviously we should attribute to those years the beginning of the new period in the history of human culture which is now called the scientific-technical revolution. One of the greatest events in this revolution has been the use of atomic energy. We all know that the consequences of this revolution may be very terrible—it may destroy mankind. In 1921 Rutherford warned me not to make any Communist propaganda in his laboratory, but it now appears that just at that time he himself together with his pupils were laying the foundations for a scientific-technical revolution.

We all hope that in the end people will have sufficient wisdom to direct this scientific revolution to the benefit of humanity.

But nevertheless the year that Rutherford died there disappeared forever the happy days of free scientific work which gave us such delight in our youth. Science has lost her freedom. Science has become a productive force. She has become rich but she has become enslaved and part of her is veiled in secrecy.

I do not know whether Rutherford would continue nowadays to joke and laugh as he used to do.

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1. Electron inertia in molecular Ampère currents (*J. Russ. Phys. Chem. Soc., Phys. Sect.* 48, 297, 1916)

2. The preparation of Wollaston fibres (*J. Russ. Phys. Chem. Soc., Phys. Sect.* 48, 324, 1916)

3. A method of reflection from crystals (*Bull. Roentgenol. Radiol.* 1, 33, 1919)

4. The Koch recording microphotometer (*Bull. Roentgenol. Radiol.* 1, 54, 1919)

5. The dependence of the emission edge of the continuous X-ray spectrum on the azimuth of the emission and the influence of the anticathode metal (*UFN** 2, 322, 1921)

6. On the possibility of an experimental determination of the magnetic moment of an atom (*J. Russ. Phys. Chem. Soc., Phys. Sect.* 50, 159, 1922; with N. N. Ssemenoff)

7. The loss of energy of an α-ray beam in its passage through matter. I. Passage through air and CO_2 (*Proc. Roy. Soc.* A **102**, 48, 1922)

8. Note on the curved tracks of β-particles (*Proc. Camb. Phil. Soc.* 21, 129, 1922)

9. On the theory of δ-radiation (*Phil. Mag.* 45, 989, 1923)

10. Some observations on α-particle tracks in a magnetic field (*Proc. Camb. Phil. Soc.* 21, 511, 1923)

11. A method of producing strong magnetic fields (*Proc. Roy. Soc.* A **105**, 691, 1924)

12. α-ray tracks in a strong magnetic field (*Proc. Roy. Soc.* A **106**, 602, 1924)

13. The Zeeman effect in strong magnetic fields (*Nature* **114**, 273, 1924; with H. W. B. Skinner)

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* *UFN = Uspekhi Fizicheskikh Nauk.*

14. The Zeeman effect in strong magnetic fields (*Proc. Roy. Soc.* A **109**, 224, 1925; with H. W. B. Skinner)

15. Over-tension in a condenser battery during a sudden discharge (*Proc. Camb. Phil. Soc.* 23, 144, 1926)

16. Further developments of the method of obtaining strong magnetic fields (*Proc. Roy. Soc.* A **115**, 658, 1927)

17. The study of the specific resistance of bismuth crystals and its change in strong magnetic fields and some allied problems (*Proc. Roy. Soc.* A **119**, 358, 1928)

18. The change of electrical conductivity in strong magnetic fields (*Proc. Roy. Soc.* A **123**, 292, 1929)

19. Magnetostriction and the phenomena of the Curie point (*Proc. Roy. Soc.* A **124**, 1, 1929; with R. H. Fowler)

20. Metallic conductivity and its change in a magnetic field (*Metallwirtsch.* **19**, 443, 1929)

21. A property of superconducting metals (*Nature* **123**, 870, 1929)

22. Magnetostriction of diamagnetic substances in strong magnetic fields (*Nature* **124**, 53, 1929)

23. The change of resistance of gold crystals at very low temperatures in a magnetic field and supra-conductivity (*Proc. Roy. Soc.* A **126**, 683, 1930; *Physik. Zs.* 31, 713, 1930)

24. Methods of experimenting in strong magnetic fields (*Proc. Phys. Soc.* 42, 425, 1930)

25. Experimental studies in strong magnetic fields (*UFN* **11**, 533, 1931; *Proceedings of the 6th Solvay Congress, Le Magnétisme*, Gauthier–Villars, Paris, p. 458)

26. The study of the magnetic properties of matter in strong magnetic fields (I and II: *Proc. Roy. Soc.* A **131**, 224, 1931; III–V: *Proc. Roy. Soc.* A **135**, 537, 1932)

27. A method of measuring magnetic susceptibilities (*Proc. Roy. Soc.* A **132**, 442, 1931; with W. L. Webster)

28. Reply to some remarks by O. Stierstadt about an error in principle in my measurements of the change in resistivity in strong magnetic fields (*Zs. f. Physik* 69, 421, 1931)

29. Hydrogen liquefaction plant at the Royal society Mond Laboratory (*Nature* **129**, 224, 1932; with J. D. Cockcroft)

30. The change of resistance of metals in magnetic fields (*Leipziger Vorträge* 1933, S. Hirzel, Leipzig, p. 1)

31. The reflection of electrons from standing light waves (*Proc. Camb. Phil. Soc.* 29, 297, 1933; with P. A. M. Dirac)

32. Liquefaction of helium by an adiabatic method without pre-cooling with liquid hydrogen (*Nature* 133, 708, 1934)

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34. Viscosity of liquid helium below the λ-point (*DAN** 18, 21, 1938; *C. R. Acad. Sci. USSR* 18, 21, 1938; *Nature* 141, 74, 1938)

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36. Expansion turbine producing low temperatures applied to air liquefaction (*J. Phys. USSR* 1, 7, 1939; *ZTF*‡ 9, 99, 1939)

37. Influence of friction forces on the stability of high-speed rotors (*J. Phys. USSR* 1, 29, 1939; *ZTF* 9, 124, 1939)

38. The study of heat transfer in helium II (*J. Phys. USSR* 4, 181, 1941; *ZETF* 11, 1, 1941)

39. Heat transfer and superfluidity of helium II (*J. Phys. USSR* 5, 59, 1941; *ZETF* 11, 581, 1941)

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41. On Professor S. Ya. Gersh's paper "Low and high pressures in deep-cooling systems" (*Bull. Mech. Engin.* 7/8, 43, 1944)

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* *DAN = Doklady Akademii Nauk USSR.*

† *ZETF = Zhurnal Eksperimental'noi i Teoreticheskoi Fiziki.*

‡ *ZTF = Zhurnal Tekhnicheskoi Fiziki.*

47. EVALUATIONS OF THE SUM OF NEGATIVE EVEN POWERS OF ROOTS OF BESSEL FUNCTIONS (*DAN* 77, 561, 1951)

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52. STATIC BOUNDARY PROBLEM FOR A HOLLOW CYLINDER OF FINITE LENGTH (*ZTF* 29, 1177, 1959; *Soviet Phys.-Techn. Phys.* 4, 1077, 1960; with V. A. Fok and L. A. Vainshtein)

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